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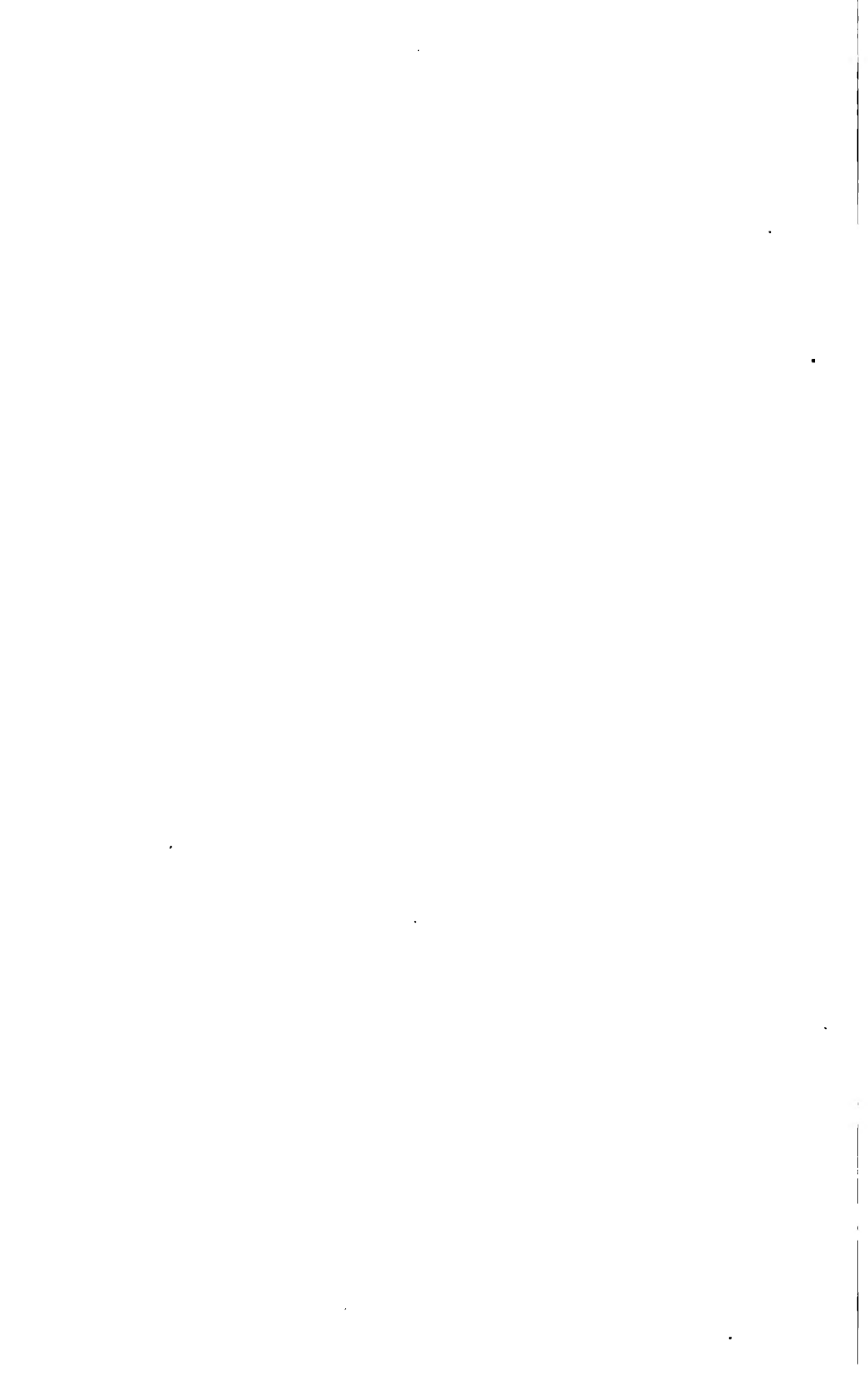
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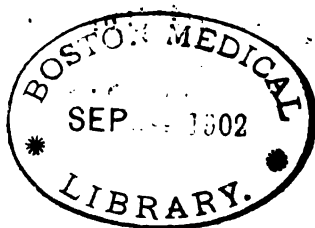


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MEDICAL SCIENCE.

PART I. ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Lessons from Surgical Practice.* By B. WILLS RICHARDSON, F.R.C.S.I.; Examiner in the Royal College of Surgeons; and Surgeon to the Adelaide Hospital, Dublin.

- I.—ENTERO-VESICAL FISTULA; MUSCULAR FIBRE AS A URINARY DEPOSIT.
- II.—DIRECT SCROTAL HERNIA WITH MULTIPLE SACS; BOTH TESTES AT THE UPPER AND ANTERIOR PART OF THE HERNIA; SUSPENSORY BANDS IN ONE OF THE SACS.
- III.—COLLES'S OPERATION FOR THE RELIEF OF STRICTURE AT THE ORIFICE OF THE URETHRA.
- IV.—INTIMATE MIXTURE OF CANCER WITH ENCHONDROMA.

Entero-vesical Fistula, with Disease of the Descending Colon, Sigmoid Flexure and upper part of Rectum—Muscular Fibre as a Urinary Deposit.

Mr. ———, aged sixty-six, suffered for thirteen or fourteen years from periodical fecal accumulations, the result of thickening and narrowing of the descending colon. According as the feculent collections formed above this portion of the bowel, the latter, as well as the distended part above, became the seat of increased pain and tenderness. To the touch the feculent mass felt globular, and about the size of a large orange. Each collection, under the use of mild laxatives and emolient enemata, gradually broke

up and was voided *per anum*, leaving nothing to be felt but the indurated and thickened gut.

Temporary relief followed the removal of the accumulation.

Every lodgment took about three months to form, and the patient was taught by experience that laxatives and enemata very much lessened his sufferings—for a time at least. Feculent masses, however, continued to form in spite of this treatment, and as the case progressed, required more active remedies for their disintegration and evacuation. Moreover, as time wore on, they formed with increasing rapidity, *pari passu* as it were, with the increasing diminution in the calibre of the bowel, and occasionally caused symptoms resembling those of internal strangulation, but which ceased when the feculent mass was discharged.

In May, 1871, the pain and tenderness in the left iliac region had become almost constant. Purpuric spots, some of which were half an inch in diameter, appeared over the body, being in largest number on the lower extremities. They disappeared under the use of lemonade, well-cooked vegetables, and some tonic medicine. At this stage of the case the countenance became anxious and the skin changed to a sallow hue.

The patient came to Dublin in August, 1871. I then found that since I had last seen him the local symptoms had received an addition, viz., a very tender tumour could now be distinguished projecting above the brim of the true pelvis, and felt as if it were wedged between the bladder and the indurated bowel, but continuous with the latter. When the forefinger was passed up the rectum as far as it could reach, I imagined that the lower part of the tumour could be at times distinguished—when, for instance, it was pressed downwards by fæces accumulated above it.

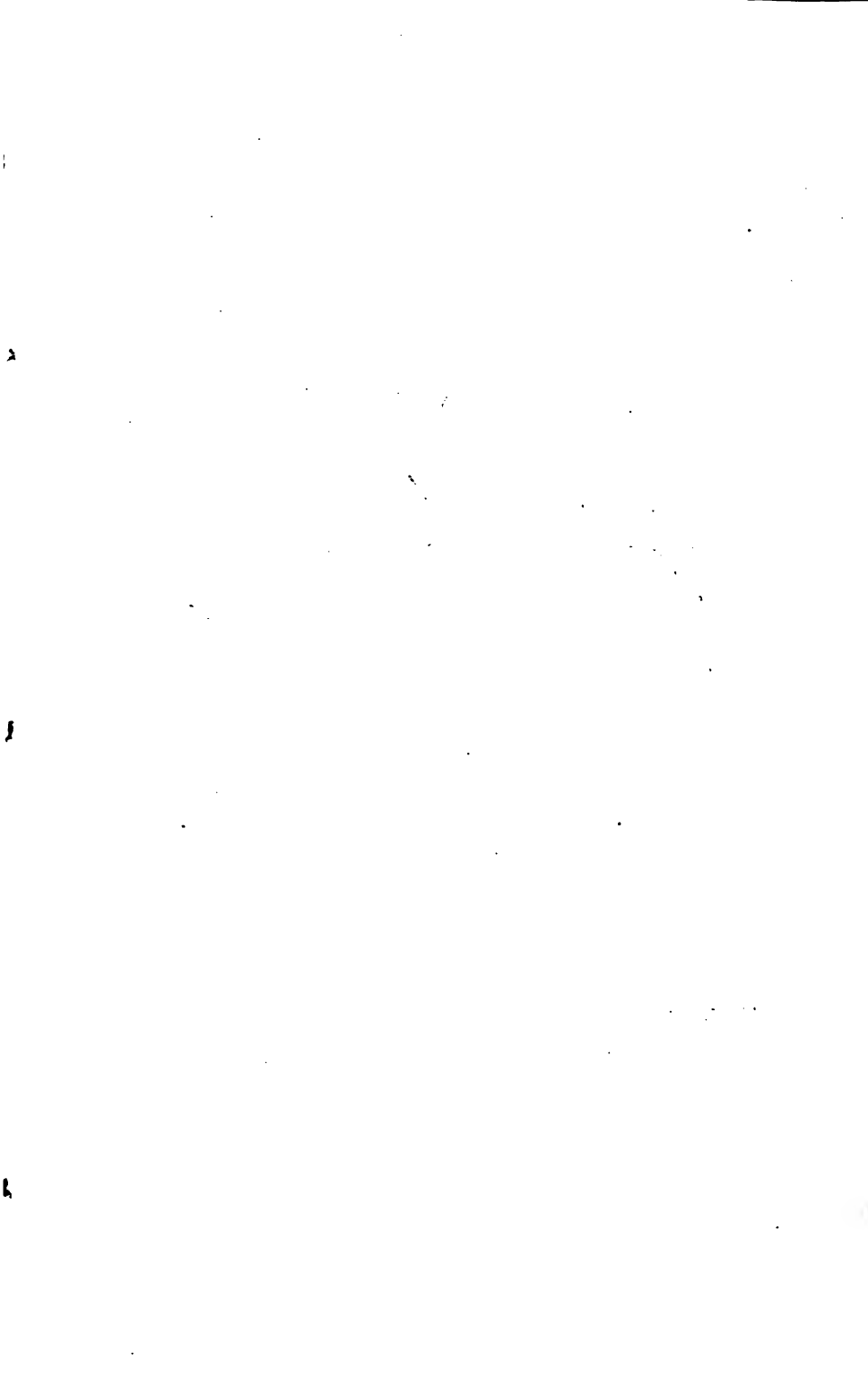
It had somewhat the feel of an enlarged prostate.

Below the tumour the rectum felt soft and healthy.

The intervals of ease were now very short, owing to the diseased bowel forming a more obstructive barrier to the onward passage of the fæces, notwithstanding that emolient enemata were cautiously administered with a soft rectum tube, procured specially for this case.

The pulse at this period averaged 68 in the minute.

Early in the following month (September) he began to complain of uneasiness at the neck of the bladder and of increased frequency with difficulty in micturition. The urine was free from sediment, and did not contain albumen. Thinking it possible that



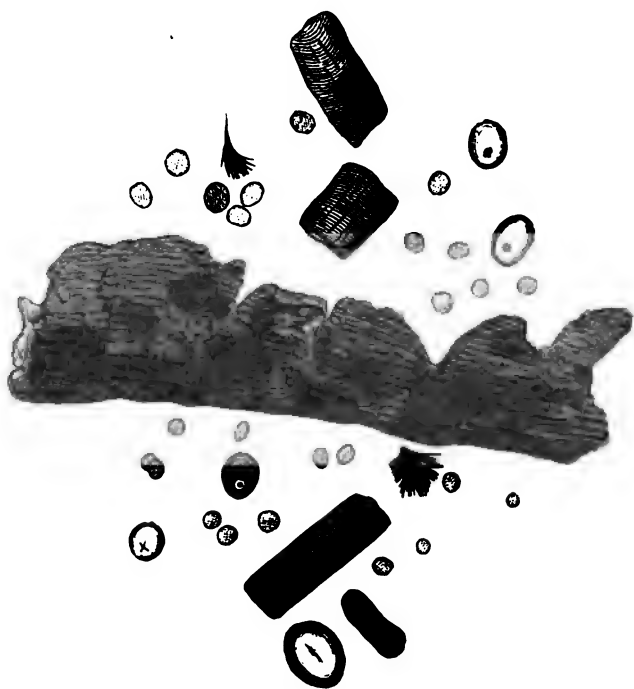


Fig. 1.—Striated muscular fibre; starch granules; feathery-looking bodies, and mucous corpuscles, from specimens of urine passed by Mr. ———. The large fibre in the centre of the figure was partially disintegrated, but striation was visible in the portion represented. Some of the fibres were more finely striated than others. Drawn with Nachet's Camera. 230 diameters.

a calculus might be present, I explored the bladder, but with negative result.

About the 4th September the bladder became exceedingly irritable at intervals during the day. Soon afterwards the catheter had to be used for the removal of small quantities of urine he was unable to expel, and the retention of which caused him much suffering.

He derived temporary ease from hypodermic injections of acetate of morphia with sulphate of atropia, and hydrate of chloral by the mouth. When thus relieved he was able to take carriage exercise. These truces were, however, very transitory, and in the intervals the frequent calls to pass water were most harassing, the urine resembling whey in appearance.

Between the 7th and 27th of September the bladder remained tolerably quiet, having been injected daily with extract of belladonna, rubbed up with tepid water. The catheter had to be occasionally used to counteract retention. It was observed on one occasion during this interval that a good deal of air was expelled *per urethram*. The existence of a vesico-intestinal fistula was therefore suspected, and the urine was again examined (27th September) for evidence on this point. As I had anticipated, it was found to contain some easily-recognized constituents of *fæces*, amongst which were: (1), undigested striated muscular fibres; (2), partially digested and disintegrated striated muscular fibres; (3), hyaline tubes, which I considered to be composed of sarcolemma, in consequence of their exhibiting some traces of transverse striation; (4), starch grains; and (5), mucous corpuscles and feathery-looking bodies (Fig. 1). These only are represented in the figure, but there was also much amorphous matter, many crystals of triple phosphate and a good deal of oil. No doubt the triple phosphate crystals may have had their origin in the urine, and the oil may have been derived from the catheter; still they are both constant constituents of the *fæces*.

Before I leave this part of the case let me observe that every precaution was taken against fallacy both in collecting and preserving the urine specimens for examination, and that striated muscular fibre was found in every specimen examined, even down to a few hours before death.

From the 27th September to the 11th October the vesical irritation was greatly alleviated by morphia suppositories, by the morphia and atropia hypodermic injections, and by vesical injections of the

extracts of belladonna and conium in water. During this interval, also, retention demanded occasional catheterism.

12th October.—Nearly pure and intolerably fœtid thin fluid fæces were passed *per urethram* this day. There were much vesical irritation and hypogastric tenderness.

15th October.—Flatus passed almost constantly *per urethram*.

23rd October.—Congestion of lower and posterior part of right lung and rapidity of respiration. Pulse quick.

15th November.—The congestion of lung subsided under the apparent influence of repeated turpentine stupes to back of thorax, and frequently repeated doses of sulphate of quinine in Burgundy wine—the wine he was taking at the time, and which he preferred.

On the 15th November he had diarrhœa, which ceased under ordinary treatment.

19th November.—The urine contained much fæces, the odour being most sickening; and the evening previously the vesical tenesmus was most tormenting. The catheter had now to be used three times daily, from his complete inability to empty the bladder. The urine had cleared in appearance by the following morning (19th November).

20th November.—Pulse 108; catheter used three times.

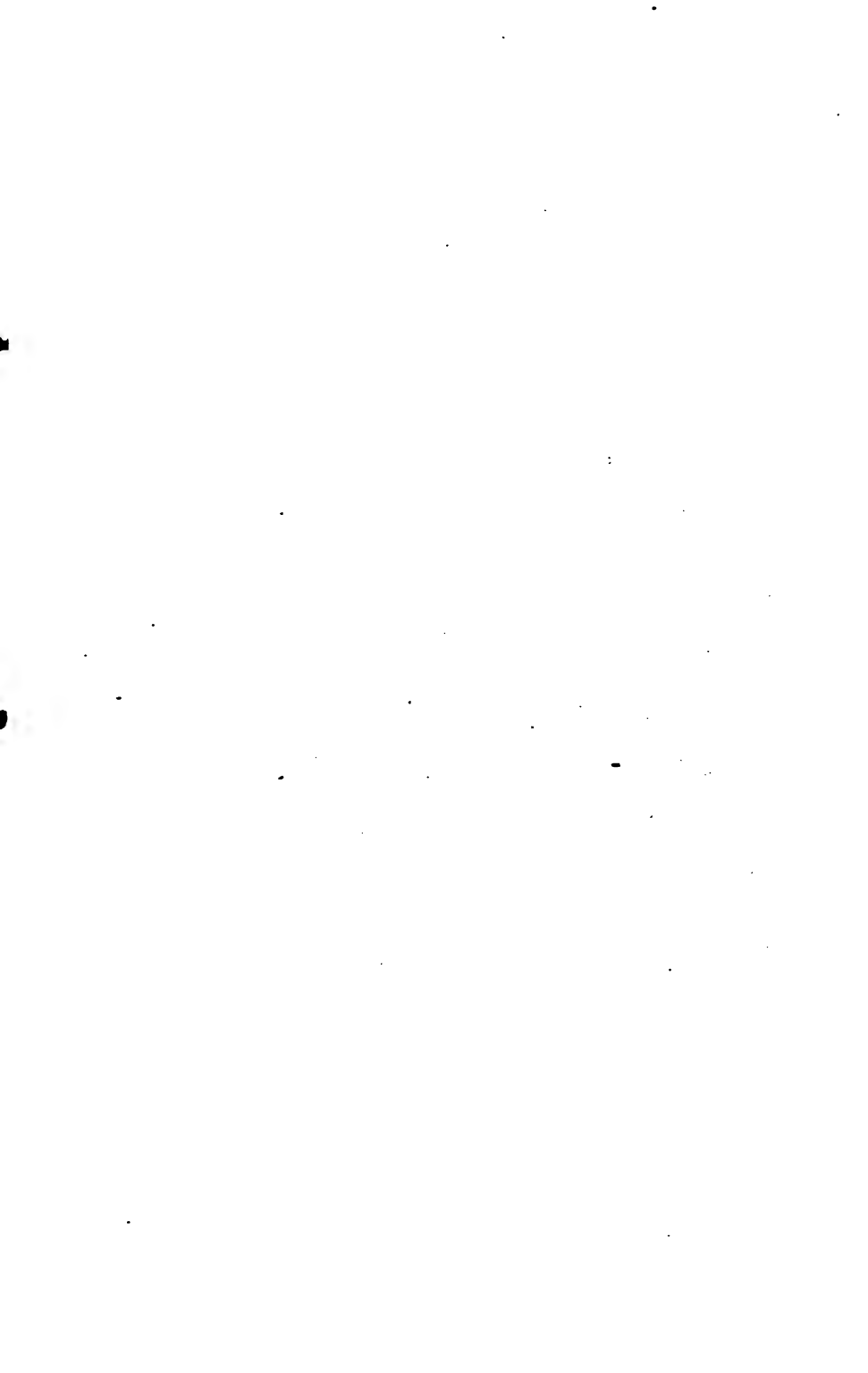
21st and 22nd November.—Pulse 120; great drowsiness, and he did not complain of the bladder. The catheter was used on three occasions during the latter day, but no fluid was found in the bladder until the third catheterism, when some disgustingly fœtid feculent urine was withdrawn.

24th November.—The pulse had gradually risen to 160, and was correspondingly weak. It gradually became imperceptible, and he died on the morning of the 25th November.

A *post-mortem* was made on the evening of same day.

The lower fourth of the descending colon was much indurated and about an inch in diameter, feeling like a rope rather than an intestine. The canal in this portion of the bowel was diminished in calibre. Below and continuous with the indurated part, the sigmoid flexure seemed to be elongated, and formed a semi-solid irregular-shaped tumour, about the size—and somewhat the shape—of a small beef kidney. This was itself continuous with the rectum, which was of normal calibre.

The tumour on section here and there was semi-gelatinous resembling fœtal brain in appearance. These semi-gelatinous portions broke up under the slightest pressure with the fingers. The



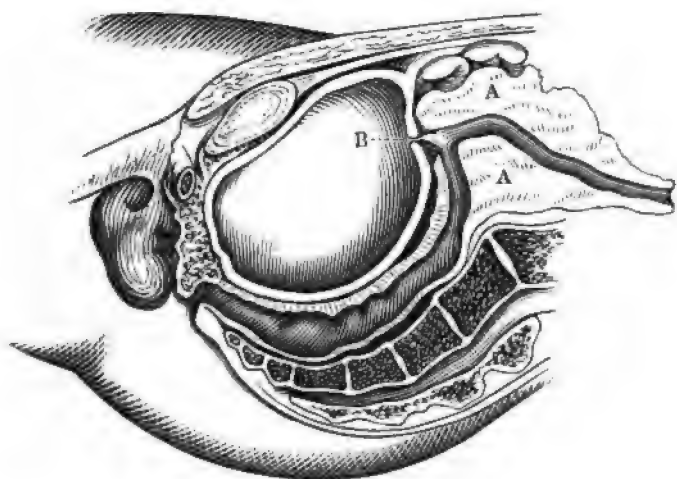


Fig. 2.—Vertical median section through the pelvis, and its contents. **A A.**—The tumour formed by the infiltrated wall of the bowel, with section of the intestinal canal passing axially through it. **B.**—Bladder, with the minute vesical opening of the bimucous fistula, or termination of the funnelled passage between the bowel and bladder.

remainder of the mass was more opaque and more solid, the whole resembling one of the forms of medullary cancer. The intestinal canal coursed along the axis of the tumour, but owing to the unevenness of the latter it was unequally surrounded by it. The lower and anterior part of the tumour was adherent to the upper and back part of the bladder, by means of an almost black, sloughy, and horribly fœtid medium, that tore under very slight traction. An ulcerated circular opening, a quarter of an inch in diameter, had formed in the diseased wall of the gut, corresponding to the centre of the intestinal portion of the adhesion. This opening led to a funnelled passage of nearly an inch in length, which passed through the wall of the bladder, and terminated on its mucous surface by a hole that barely allowed a fine probe to enter it (Fig. 2). Through this canal the gaseous and semi-fluid contents of the bowel were enabled to pass from the latter into the bladder, the mucous membrane of which was intensely inflamed, and of a dark mulberry colour.

Cruveilhier mentions that it is not rare to find a knuckle of small intestine, or the sigmoid flexure of the colon adherent to the summit or to the posterior wall of the bladder. There are not many cases, however, to be found in our records in which a fistulous passage existed between this portion of the bowel and the bladder. Cruveilhier alludes to one case, indeed, of adhesive union of the sigmoid flexure with the bladder. It occurred in an old woman:—"The walls of the bladder were entirely wanting corresponding to this adhesion." There was no direct communication between the bladder and bowel, for he observes that it "is probable that the gangrenous inflammation which had destroyed the whole thickness of the bladder would have soon destroyed the corresponding wall of the colon, if death had not occurred."* He also mentions that in "man a recto-vesical fistula might be produced by an abscess situated between the rectum and the bladder, opening simultaneously into those cavities;" but usually these entero-vesical fistulæ "are the consequence of alteration which propagates itself from the rectum to the bladder. The sigmoid flexure or any other portion of the cancerous intestine may likewise open into this, which organ has itself become cancerous."^b

In the third volume of the *Proceedings of the Pathological*

* *Traité D'Anatomie Pathologique Générale.* Par J. Cruveilhier. Tome deuxième A Paris. 1852. P. 533.

^b *Ibid.*

Society of Dublin (1st series, page 300),* there will be found the description of a very interesting case of recto-vesical fistula that was exhibited by Dr. Banks at the meeting of the Society held on the 20th February, 1858. In this case, also, fæces were passed *per urethram* during life. The middle portion of the rectum was "closely bound by old adhesions to the bladder, and contracted to so small a size that a catheter could not be passed through the gut. On cutting into the bladder its mucous coat was found to be much thickened, deeply ulcerated, and in part coated with ash-coloured lymph. Immediately behind the trigone an opening existed between the bladder and rectum, through which a large-sized catheter could readily be passed; this opening was between, but posterior to, the mouths of the ureters. The inside of the bladder closely resembled a piece of ulcerated intestine; the upper and lower portions of the rectum were also extensively ulcerated.

The symptoms observed in this case were:—Difficulty in micturition; the passage of fæcal matter and air *per urethram*; the urine being never unimpregnated with fæcal matter; dysenteric evacuations, and, at times, costive bowels, at which periods the greatest amount of fæces passed by the urethra. Frequently an enema, soon after being thrown up into the rectum, was ejected through the urinary passages, and then followed casts of the urethra; but in general the excrement was fluid. Flatulent distension of the bladder. Retention of urine occurred occasionally, and was often the result of the presence of some solid body in the urethra. On one occasion a piece of yellow elastic ligament, which had been taken in soup and had found its way from the intestine to the bladder, was arrested in the urethra. and caused much distress. Mucopurulent discharge from both rectum and urethra. One day he was seized with severe rigors and retention of urine, when it was found that an obstruction existed to the passage of the contents of the bladder by the urethra. The pain felt at the seat of the foreign body was very acute. After many efforts, and with much trouble, a piece of bone, presenting many sharp points, was extracted from the urethra. The urine eventually passed away involuntarily *per anum*, very little coming by the urethra. At last he sank exhausted, worn out by this terrible malady, combined with thoracic and other complications.

* For the early history of this case see the *Dublin Hospital Gazette*, Vol. iii., p. 209.

In the description of Mr. ———'s case, I mentioned that his symptoms received an addition by the formation of the tumour discovered in August, 1871, projecting above the brim of the true pelvis. It might be argued that this tumour, formed in great part by the sigmoid flexure, commenced simultaneously with the induration of the bowel above it. I am myself inclined to the opinion that it was a superadded disease, because for years after the discovery of the indurated colon the patient remained unaltered in appearance, and enjoyed life as soon as he got rid of each feculent lodgment. I date the commencement of the cancerous disease to either the end of 1870 or the beginning of 1871, when emaciation first set in, when he altered in colour and lost strength rapidly; in fine, when his malaise was almost constant—a condition of health which leads us to suspect the existence of malignant disease when occurring either in or after middle age.

Direct Scrotal Hernia, having primary, secondary, and tertiary Sacs—Several inches of the protruded Bowel suspended in the Sac proper by radiating Bands, prolongations of one long Band that descended from the Abdomen through the Ring—Both Testes at the upper and anterior part of the Tumour.

J. A., aged fifty-five years, a very pale cachectic man, was admitted to the Adelaide Hospital on the 20th November, 1871. He was a cook on board a vessel, and while raising a very ponderous iron box full of coal, his rupture was suddenly forced from the groin into the scrotum. The taxis, and the inverted position were tried for its reduction, but with only very partial success; still he was able to return to his occupation after a few weeks, with the hernia supported by a bag truss. As time elapsed, however, it gradually enlarged.

When he came into hospital the hernia was fourteen inches in its longest or vertical diameter, and twenty-four inches in circumference at its widest part (Fig. 3). If, however, he changed from the recumbent to the sitting posture, the tumour, being unsupported, it increased so much in size as to reach nearly to the knees, and its surface became very uneven. This unevenness was at the time attributed to pressure by the intestines against the walls of a thin and thinly covered sac, but was subsequently found to be produced by the secondary and tertiary sacs hereinafter mentioned.

The testes were situated nearly at the same level on the upper

and anterior part of the hernia. The cord of the right testicle could be easily felt passing backwards and inwards towards the root of the penis.

About a third of the hernia was reducible, and when this was returned into the abdomen, the remainder felt like slender sausages in the scrotum. There was fluctuation in the most depending parts of the abdomen, and as the hepatic region was abnormally clear on percussion, the existence of cirrhosis of the liver was suspected. The hypogastrium was tender when pressed.

Castor oil and compound tincture of rhubarb effectually opened the bowels.

From the 20th November to the 8th of January, 1872, there was scarcely any abdominal uneasiness, with the exception of occasional flatulent distension, which a carminative draught usually relieved.

On the 9th of January the area of the hypogastric tenderness had enlarged, and tympanitis was distressing. Bran poultices were applied to the abdomen, and some carminatives given. The next day there was constipation, and he vomited once. A little above the right abdominal ring there were increased pain and tenderness. The abdomen was larger, apparently from augmented peritoneal effusion and tympanitis. The hernia was soft and free from tenderness; the pulse 80, and the tongue natural. He had a turpentine and asafœtida enema with the long tube, and a grain of opium in pill twice daily. The 12th of January he felt better; the enema brought away a good many feculent masses. The hernia continued soft, the pulse quiet, and the tongue natural. The enema and pills were repeated.

13th January.—The enema brought away only a few scybala. Although the hernia continued soft and communicated an impulse to the fingers when the patient coughed, it was less reducible and more painful during the cough. The tenderness above the ring, however, had lessened. Hypogastric and iliac regions painful. Stomach quiet. Turpentine and asafœtida enema, and repetition of the pills.

15th January.—Some scybala were voided with the enema. Occasional hiccup and bilious eructations. Tongue natural. Pulse 100. Abdominal fluctuation very pronounced. He turned in bed without pain, except in the hypogastrium, where also pain was produced by coughing. Hernia larger but not tender, and had become very transparent anteriorly and at the left side. The

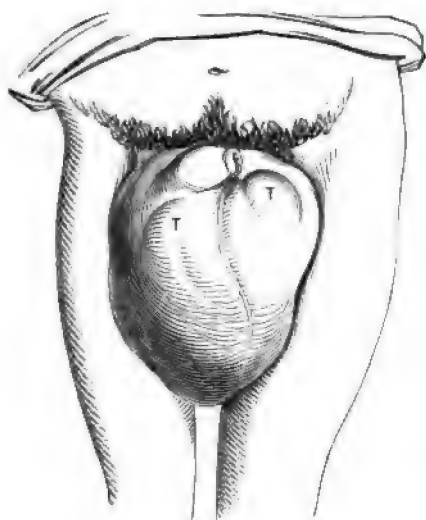


Fig. 3.—Right direct inguino-scrotal hernia, with the testes on the upper and front part of the tumor. T T.—The testicles. The drawing was made while the man was in the horizontal position.

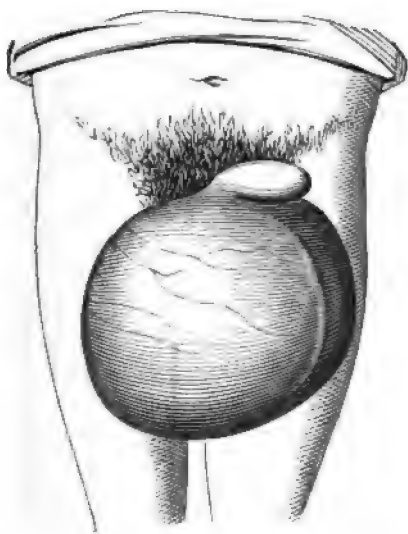


Fig. 4.—Hernia on the morning of the 17th of January.



penis appeared buried in the tumour. Some of the hernial contents being still reducible, it was considered that the symptoms depended rather upon latent, probably cacoplastic, inflammation of the hernia and the abdominal peritoneum, than upon strangulation.

A grain of opium with a drop of creosote twice daily; the constant application of bran poultices to the belly; ice, to relieve thirst, and a turpentine enema.

16th January.—Had a good night; pulse 96; constipation; hiccup more frequent, and occasional bilious vomiting; tongue natural; pain and tenderness in the hypogastrium diminished; hernia larger, much more tense, and of anything more transparent. It was neither œdematous nor tender to pressure. In the evening hiccup and vomiting had ceased.

The great amount of fluid in the herniary sac rendered it so tense and heavy that the patient felt greatly alarmed lest it should burst. Measured with the calipers, it was found to be twelve inches in its greatest antero-posterior diameter, and twenty-six inches in circumference; and it almost reached the knees. To lessen the distressing feeling of tension, I tapped the sac at 11.30 o'clock p.m. in its most translucent part anteriorly and to the left side, and withdrew three pints of an albuminous whey-like fluid. This was immediately followed by flaccidity of the hernia. Pulse 104.

Ordered $\frac{1}{4}$ gr. of acetate of morphia hypodermically.

An hour after the tapping the hernia was perceptibly re-filling, and in some hours became larger than it had been before. The abdomen at the same time decreased in size.

17th January.—Had an uneasy night, and hiccup again recurred. The hernia measured twenty-nine inches in circumference at its widest part, and *now* had the appearance of an enormous œdematous infiltration (Fig. 4). I made several punctures and one depending incision in the infiltrated tissue, which allowed free drainage of the infiltrating fluid. The abdomen was more reduced in size, and fluctuated very indistinctly, which led me to suppose that both the peritoneal and hernial fluids were escaping, as soon as secreted, into the scrotal areolar tissue, and thence partially through the punctures and incision.

In the left side of the abdomen, a leather creak, quite different in feel from the emphysematous crackle indicative of mortification, could be developed by pressure. Pulse 96, and full; tongue natural. A turpentine and asafœtida enema brought away some

scybala. At 9 o'clock p.m. the hiccup, which had ceased for several hours, returned. Hernia of vast size. Less draining through the punctures. It was now so tense and hard at the ring, we thought it advisable to explore this region by operation. Accordingly, assisted by my colleagues, Drs. Walsh and Barton, I cautiously dissected down to the ring, the patient being fully under the influence of chloroform. The neck was then found so tense and unyielding that I opened it by a short incision. A good deal of whey-like fluid gushed out, and small intestine was exposed. It was intensely inflamed, and covered by yellow purulent lymph, which cemented its coils together. The abdominal ring was very large, and allowed three fingers to be passed into the abdomen alongside the mesentery and intestine, which were not compressed in the slightest degree by it, the tension having been caused by fluid pressure. When we reflected for a moment upon the enormous size of the protrusion, and its hopelessly inflamed condition, the bowel was no further exposed, and the wound was united by sutures.

Opium treatment continued.

18th January.—Bilious vomiting three times after the operation. Increased thirst and frequent hiccup. Pulse 108; tongue natural; bowels unmoved. Had but little sleep, and the countenance was more sunken. Hernia measured thirty-one inches in circumference at its widest part.

An opiate pill with creosote every fourth hour; ice; whiskey and champagne. Hiccup recurred in the evening, but there was no return of the vomiting; belly soft and not tender; pulse 124.

19th.—Slept a good deal; stomach continued quiet; pulse 110, weaker; constipation; tongue natural. Coughing caused impulse in the upper and anterior part of the tumour. Death took place on the following morning.

The *post-mortem* was made on the 21st January, when pathological anatomy was enriched by a most curious, instructive, and rare specimen of hernia.

On laying open the abdomen the following appearances were noticed:—(1.) Intense injection of both the lining and investing peritoneum, which was more or less concealed by a coating of yellow purulent lymph. (2.) Great thickening of the peritoneum lining the anterior abdominal wall, and evidently of some standing. It was the probable seat of the leather creak felt during life. (3.) The lower edge of the great omentum was turned upwards

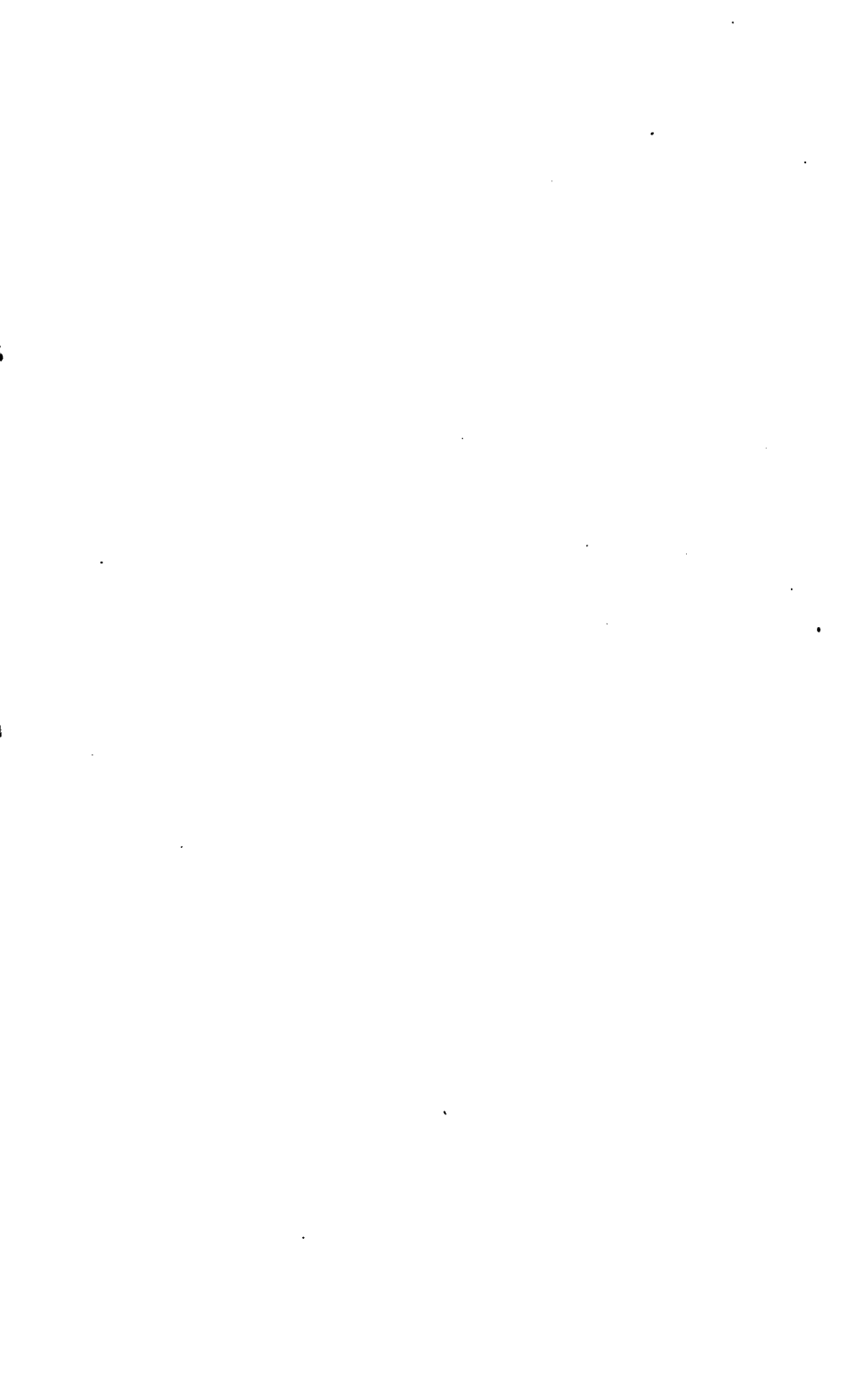




Fig. 5.—The hernia intestine, with the radiating bands attached to the portion of bowel found in the sac proper. The fingers grasp the long single band at its termination in the fibrous nodule.

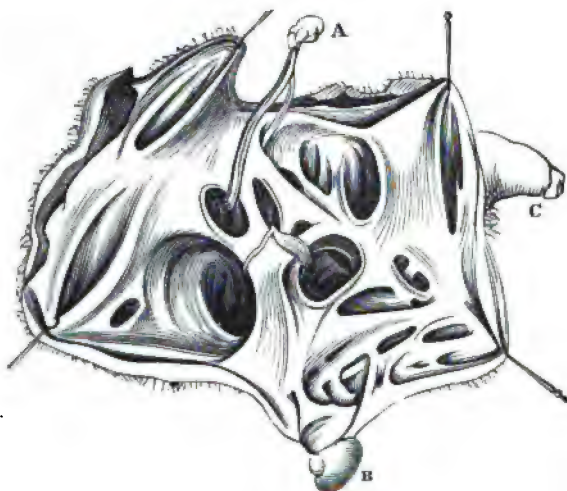


Fig. 6.—Sac proper, laid open, and moderately stretched with hooks to expose all the openings and depressions. A.—A second fibrous nodule from which two fibrous bands passed, each to enter a secondary sac, where it was lost on its wall. B.—Right testicle which in the figure seems as if it were originally below the hernia. This position in the drawing is owing to the incision having been made above it, so that when the margin of the cut wall was turned down for the artist, the testicle passed with it. At the time of death the testicle was found adherent to the middle and anterior part of the hernia, having descended from its first position as the rupture enlarged. C —Penis.

towards the diaphragm. (4.) A small quantity of whey-like fluid, with yellow lymph flakes, in the most depending portion of the abdominal and pelvic cavities. (5.) A very nodular, hard, and contracted cirrlosed liver; and (6) an enlarged spleen.

The hernia of the ordinary direct variety was formed of mesentery and all the small intestine, excepting the first four feet and a few inches between the ring and the cœcum. A portion of the non-hernia jejunum for about two inches was diminished in calibre to that of a swan quill, but without induration. The external abdominal ring, as disclosed at the time of the operation, allowed three fingers to pass without impediment alongside the uncompressed mesentery and intestine from the abdomen into the hernial cavity. When the latter was laid open it was discovered that instead of being a single sac it was composed of a primary sac or sac proper, of secondary, and of tertiary sacs. All of these contained fluid similar to that found in the abdomen. The longest portion of intestine was in the sac proper, and the remainder in secondary sacs, nearly every one of which had its portion. The mesentery was much thickened.

The bowel in the sac proper was suspended both by its mesentery and by a series of fibrous bands arranged thus:—A long firm fibrous band descended from the portion of mesentery remaining in the abdomen, along with the intestine and mesentery, through the external abdominal ring, into the sac proper. After passing a little distance downwards in front of the intestine it enlarged into a dense fibrous nodulated mass, having the size of a walnut (Fig. 5). From this nodule thirty-one separate fibrous bands radiated downwards to the convexity of the intestine, where having arrived, each band spread out into a fan-like form, and was inserted into the bowel opposite to the insertion of the mesentery. None of the intestine was strangulated, but the whole partook in the general inflammation. There were a few dark patches in the mesentery of the portion of bowel in the sac proper.

None of the gut passed between any of the bands.

When the intestine was removed the sacs were then examined. The *sac proper* (as I have called it for clearness of description), was of large size and alone communicated directly with the abdomen. Its wall was perforated with innumerable openings, both oval and round, of various dimensions, and which, with the intervening thickened structure, gave to it the appearance of the inside of an enormous highly columnar cardiac ventricle (Fig. 6). Several of the openings led

into secondary sacs, two of which were likewise highly columnar. The two largest of these secondary sacs would each hold a good-sized orange. In the wall of the sac proper there were depressions, each of which, like the openings, had a thickened margin or rim. The largest secondary sacs were situated at the left side of the sac proper, both behind and below the left testicle, which was pushed forwards and raised by the uppermost one; indeed, this sac, it is probable, at its early formation, insinuated itself behind the right testicle also, and tilted it forward in its course to the left side. The walls of the largest secondary sacs were perforated by openings, and all were marked with depressions; the openings led into distinct pouches or tertiary sacs, most of which were immediately under the skin, and one of which was probably the seat of the tapping on the evening the three pints of fluid were removed from the sacs. The sacs were lined by a smooth serous-looking membrane, of which the walls of the tertiary sacs seemed to be alone composed.

It may be of interest to record the measurements of the orifices leading into the chief secondary sacs—one of these, an oval opening, measured three and a-half inches in its long diameter, and three inches in its short diameter; and the second, a circular opening, measured two inches in diameter. The pouch into which it led was sub-divided into two pouches by a complete fibrous septum, one of which pouches was intersected by a strong fibrous band having its extremities only attached to the wall of the pouch. Finally, a third opening, circular also, measured one and a-half inches in diameter, and led into a secondary sac somewhat smaller than those just mentioned. The following diagram will assist the reader in understanding the relations of the sacs to one another.

There was no evidence of a hydrocele of the tunica vaginalis.

It would be rash to affirm that herniæ similar to this one have not hitherto been observed. Nevertheless, that a multilocular sac, with an accessory mesentery, of pathological origin, assisting the ordinary mesentery to suspend the intestine in one of its divisions, must be an occurrence of extreme rarity, is evident from the fact that I cannot find anything analogous described in the writings of Garengéot, Pipelet, Pott, Hey, Sir Astley Cooper, Samuel Cooper, Lawrence, or even in Holmes' Surgery. Sir Astley Cooper, no doubt, has illustrated a case of umbilical hernia, in which there were four holes in a hernia sac, through two of which omentum passed outwards from the sac into spaces or secondary sacs outside it. In another case (umbilical also) several pouches were found on

the wall of the sac, in one of which a separate portion of the colon had been strangulated. He makes no mention of tertiary sacs.

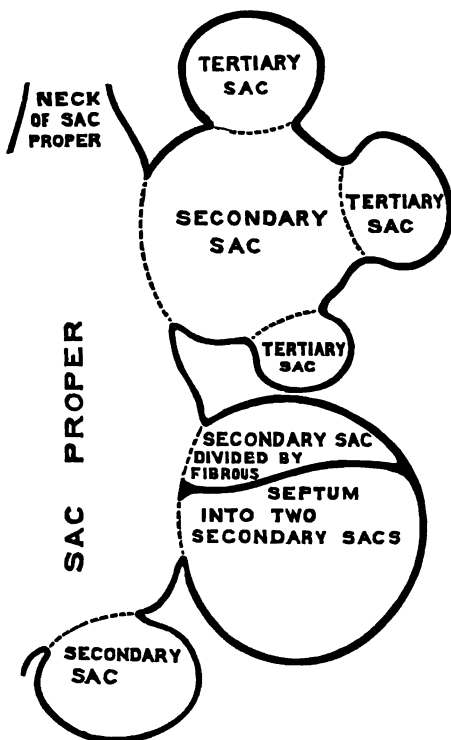


Fig. 7.

Cloquet,^a however, in his excellent description of multiple hernial sacs, describes and illustrates primary and tertiary sacs occurring in the same hernia. In one of his cases (42nd observation) the sac was unequal, having protuberances on its exterior, the neck was round, fibrous, thick, very resisting, and was continuous with other prominent obliquely descending bands, which were attached to its wall, and which were similar to it. An inch below the neck there was a large round cavity, the entrance to which was bordered by a rim. This cavity was directed upwards and outwards, and terminated in three *culs-de-sac*.^b Below it there were two other *culs-de-sac*, the entrance to each of which was provided with a

^a Recherches sur les Causes et L'Anatomie des Hernies Abdominales. Par Jules Cloquet. Paris, 1818.

^b Ibid., Plate II., Figs. 4 and 5.

fibrous rim. One of these formed a species of cœcum, its cavity being very irregular, and gave insertion to elongated, round, reddish filaments formed by omentum. The fundus of the sac was united to the tunica vaginalis, and presented nothing particular; the vas deferens adhered very intimately to the posterior aspect of the tumour.

There are two modes by which the formation of the multilocular hernial sac may be explained. If, as Cloquet supposes, the orifice of the sac adheres intimately to the ring, the adhesions oppose the locomotion of the peritoneum lining the abdominal walls, and the portion of this membrane which forms the sac becomes distended, thinned, and partially ruptured. The sac is thereby dilated, its walls are frayed in numerous places, and covered by whitish, fibrous, reticular filaments, united by a fine and transparent pellicle.* A little further pressure from the contents might readily force out this fine pellicle here and there between the reticular filaments, which would ultimately become secondary sacs. Cloquet seems to have thought that the multilocular sac is formed in many cases by sacs that descend successively by the same ring beside one another, all of which communicate with the abdomen by means of a common opening; hence results a sac composed of several secondary cavities.^b

In his description of the first-mentioned kind of hernia he says that in some cases the displaced peritoneum does not distend in all its points; its frayings are only partial, and the sac becomes irregular with protuberances, thinner in certain situations than in others. "It is, therefore, by the distension and fraying of the weakest portions of the hernia sac, that most of the bulgings and certain secondary cavities are formed. This diminished resistance which it offers in certain situations may depend upon the disposition of the peritoneum itself, or on that of the neighbouring parts. Under the influence of an equal pressure the most feeble points, or the least supported, yield more, and enlarge the cavity of the sac in several directions by forming species of appendages, the depth of which varies. The peritoneum all round the part which has yielded returns upon itself, becomes thicker, and constitutes a form of circular ring or neck. The sac thus distended unequally is thinner and less susceptible of resistance at the situation of the depressions than in the remainder of its extent."^c

* *Ibid.*, p. 46.

^b *Ibid.*, p. 63.

^c *Ibid.*, p. 46.

The latter explanation of the mode by which secondary sacs may be formed appears to me to be more applicable to the case of J. A.— than the view which would refer it to “ successive locomotions,” to use the phrase of Cloquet, of the peritoneum from the abdominal ring.

The tertiary sacs were probably more recent formations than the secondary sacs, and also probably were the result of fluid pressure against the walls of the secondary sacs, in the same way that the bladder becomes sacculated from urine pressure. Pouches are formed sometimes at the abdominal rings by the pressure of ascitic fluid, and therefore this explanation of the tertiary sac formation is by no means improbable.

None of the writers that I have mentioned, Cloquet not even excepted, describe a fibrous band having radiating processes like those illustrated by me (Fig. 5). As this band passed through the ring with the intestine it may be assumed that it, together with the radiating suspensory fibres, originated in an old attack of peritonitis previous to the descent from the abdomen, of the portion of bowel to which they are attached. This opinion is strengthened by the fact that the peritoneum of the anterior abdominal wall was much thickened from peritonitis prior to the fatal attack.

I cannot find a case recorded in which such a disposed band, with radiating processes, existed in combination with a sac proper, and with secondary and tertiary sacs in the same hernia.

The uneven appearance of the rupture when the patient changed from the horizontal posture, and which, without the evidence afforded by a *post-mortem* examination, would have been attributed to intestinal pressure against the walls of a thin sac, resulted, on the contrary, from distension of the secondary and tertiary sacs by their fluid contents.

Although the *post-mortem* examination proved that in this case the simulation of strangulation followed upon serous inflammation, nevertheless cases of this kind are calculated to cause us great anxiety and embarrassment. “ The want of tension,” says Lawrence, “ and of pain at the ring, while the swelling itself is painful, and a previous attack of feverish rigor might lead us to suspect inflammation of the hernial contents. If the ring afterwards becomes tense, and the included parts considerably painful, we should conclude that strangulation had supervened, and act accordingly.”*

* A Treatise on Ruptures. By W. Lawrence, F.R.S. London: 1838. Pp. 66.

In this case the neck of the sac became almost suddenly tense and painful, and yet this was caused by fluid pressure, and not by strangulation, and is an exemplification of the difficulty we meet with in attempting to lay down unerring rules suitable for every case of hernia requiring active interference.

In conclusion, I think it will be conceded that I have not gone too far in my assertion that the case of J. A—— is one of the most curious, instructive, and valuable specimens of hernia on record. This will be still more evident from the following summary of its peculiarities:—

1. Its enormous size, although a direct hernia. Lawrence never met with a direct hernia that reached the bottom of the scrotum, and Sir Astley Cooper never saw one of more than moderate dimensions.

2. The passage from the abdomen, through the ring, of a fibrous cord, which divided into thirty-one suspensory bands, which were attached to the intestine in the sac proper opposite to the insertion of the mesentery.

3. The multilocular nature of the sac, being composed of a primary sac, and several secondary and tertiary sacs.

4. The position of the testicles on the upper and anterior part of the tumour, and the passage of the right spermatic cord upwards and backwards on the inside of the partially developed neck of the sac proper to the ring, its situation in most instances being on the external and posterior side of the sac. This disposition of the cord differs from that of the case described by the late Mr. Todd in the 1st vol. of the *Dublin Hospital Reports*. In his case—a direct hernia by-the-by—the testicle was situated behind the sac, and the spermatic cord extended, in an undivided state, across in front of the upper part of the sac, to its pubal side, and descended on that side to the posterior surface of the sac to reach the testicle. In this way it formed a sort of arch, embracing the neck of the sac for nearly two-thirds of its circumference. The reader will please observe that it was the *whole* cord that was situated abnormally in my patient, as well as in the case dissected by Todd. It is so rare to find any portion of the cord abnormally situated in a direct hernia that even Erichsen, in the recent edition of his "*Science and Art of Surgery*," says that it never occurs.

5. The translucency of the greater portion of the tumour, which

enabled me to remove three pints of fluid indirectly from the sac proper by tapping one of the accessory sacs.

6. The sudden tension of the neck of the hernia from the pressure of fluid within it. In ordinary doubtful cases of inflamed hernia this alteration in the condition of the neck is considered to justify operation, but in cases like that of J. A—— it would be deceptive.

7. The inversion of the omentum.

8. The narrowing of the jejunum.

9. Finally, it may be fairly assumed that the condition of system consequent upon the cirrhosis of the liver had a deteriorating influence in the evolution of the low form of inflammation that occurred in this case.

Colles's Operation for the Relief of Stricture at the Orifice of the Urethra.

Whatever difference in opinion may exist as to the most advisable treatment of stricture seated in the urethra posterior to the orifice, there is no question that ordinary dilatation, divulsion, and even rupture, are not suitable for the cicatricial constriction that follows upon circular or ring chancre engaging this opening.

To remedy this intractable form of stricture a cutting operation seems to be essential, and then comes the question :—What operation would most likely conduce to the permanent enlargement of the orifice ?

I think it questionable, whether mere division of the stricture with the knife is often followed by permanent cure, such a cure as would enable the bougie to be altogether laid aside.

It is not thus with the operation devised by the late Mr. Colles for remedying this stricture, which, as he observes, “does not in any instance admit of a cure by the ordinary treatment of strictures.”*

The following is Mr. Colles's description of the operation :—

“Having detached the skin from the end of the urethra, to which it is generally intimately adherent, I divide the urethra below, to the length of half an inch, I raise the mucous membrane from each lip of the incision, then cut away a portion of the bared corpus spongiosum, to such an extent as will allow the raised mucous membrane to cover the cut edge. I stitch down this membrane upon the corpus spongiosum, and thus, having covered each lip of

* Practical Observations on The Venereal Disease, and on The Use of Mercury, by Abraham Colles, M.D. London, 1837. P. 95.

the wound by mucous membrane, I have effectually guarded against the possibility of re-union of the lips of the wound, or subsequent contraction of the opening."

The permanent benefit derivable from the operation has been proved by cases occurring in Dublin as well as by cases of Ricord, and of Weber, whose operations for stricture of the orifice left after amputation of the penis, and for congenital narrowing of the orifice, is the same in principle as that of Mr. Colles.

This operation having been overlooked by many authors of standard works on Surgery, induces me to place the following evidence of its efficacy on record, particularly as it has been tested by time, the only criterion by which a question of this kind should be decided.

J. A., aged thirty-two years, came under my observation on the 21st of June, 1871, and stated that five years previously he had a clap together with chancre that surrounded the urethral orifice. On examining the glans, there were cicatricial depressions consequent upon other sores, although, as well as he could remember, he had the sore at the orifice only.

The prepuce somewhat resembled the prepuce of hypospadias, still the contracted orifice (Fig. 8) was nearly in the axis of the normally situated opening.

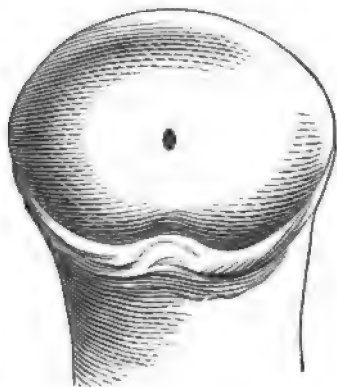


Fig. 8.—Glans penis and the contracted orifice, which was surrounded by dense cicatricial substance.

Where the prepuce was deficient, laterally and inferiorly, there were unmistakable evidences of previous ulcerations. A good deal of gleety discharge flowed from the urethra.

22nd June.—With considerable difficulty I passed a small bougie

through the orifice and on to the bladder. The point of this instrument is not more than No. $\frac{1}{2}$ of Weiss's gauge.

On this occasion evidence was afforded to the touch that the contraction extended from the orifice backwards for about an inch or a little more.

Considering the length of the contraction I determined, in the first instance, to enlarge the orifice according to Colles's method, and when the wound had been healed for a few weeks, to divide the remainder of the stricture either with the *bistouri caché* or the urethrotome.

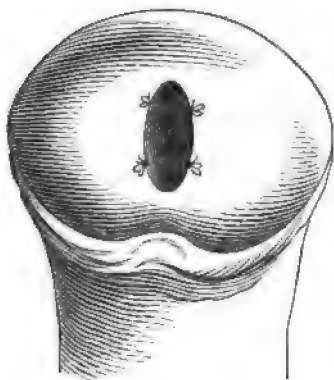


Fig. 9.—Shape of the orifice and the funnelled passage formed by the operation.

Colles's operation (Fig. 9) was performed on the 24th June, 1871, the patient being fully narcotized by chloroform.

The operation was tedious, the urethral lining membrane having to be dissected from the corpus spongiosum very slowly, so that the bleeding caused by each cut might cease before making another. This is an important precaution to take in the operation, lest the lining membrane, when concealed by blood, be hopelessly injured by the knife or the scissors. One small artery in the site of the frænum was divided and had to be tied.

When a sufficient amount of the corpus spongiosum had been removed, and bleeding had ceased, the edges of the partially detached urethral lining membrane were everted and secured in contact with the margin of the cut glans by means of four fine silk interrupted sutures (Fig. 9).

The healing process was so rapid, I was enabled, although reluctantly, to allow the patient to return to the country in a few

days, where he had to give evidence on a trial at the assizes. He promised to come to Dublin, at the end of three months, for the completion of his treatment. This, however, he failed to do, and I did not see him again until June, 1872. This procrastination, on his part, was, in regard to the value of this record, rather an advantage than otherwise, a year being a much more satisfactory test of the operation than an interval of three months.

At this date the condition of the orifice was most satisfactory. It had the appearance of an antero-posterior slit (Fig. 10), and looked

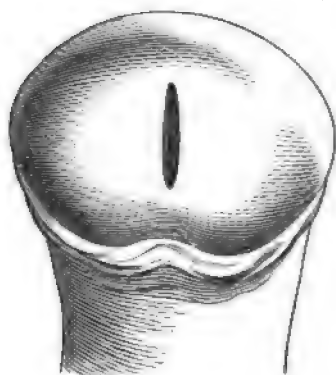


Fig. 10.—Orifice twelve months after the operation.

so natural that were it not for four small red suture marks in the glans near its margins few would suspect it to be of artificial formation.

27th June, 1872.—A No. 13 bougie (Weiss's gauge) could be passed with ease down to the posterior contraction, which was three quarters of an inch from the orifice. I had no difficulty in passing a No. 6 conical bougie through the contraction, and on into the bladder. As the urine deposited a good deal of ropy mucus, some benzoic acid, ergotine and extract of hemlock were ordered in pills. These were changed on the 28th for a mixture composed of infusion of pareira, tincture of hyosciamus, and dilute nitro-hydrochloric acid.

29th June.—I divided the posterior contraction from behind forwards with a small urethrotome, a grating sound being audible while the blade was cutting the stricture. The blade felt to me as if it were dividing gristle. A No. 12 black bulbed catheter (*à demeure*) was introduced, and he was given a full opiate.

Contrary to directions he removed the catheter in three hours.

A very trifling bleeding followed upon the operation. In the evening a rigor ushered in a slight attack of urinary fever, of which there were no traces on the 1st of July.

A No. 13 bougie was passed on the 1st, 2nd and 3rd of July. He then rebelled against the further use of instruments, so that it is probable, unless he resumed their use after he left hospital, a relapse of the posterior stricture is exceedingly probable. This, however, is immaterial in the question as to the value of Colles's operation in permanently enlarging the orifice of the urethra.^a

It must, I think, be conceded that a year's test of this method is sufficient; and as no re-contraction of the funnelled orifice has occurred to diminish its calibre to less than No. 13 of Weiss's catheter gauge, it may be considered a most satisfactory trial of the method.

At a meeting of the Surgical Society of Ireland, on the 17th of April, 1841, the late Mr. Robert Williams gave the histories of two cases in which he performed Colles's operation. With these exceptions there will be found but little evidence of its having been performed by others, which is surprising when we consider that it is both an easy and certain cure for this most distressing affection.

Mr. Williams's operations were performed on the 21st January, and 4th February, 1841, and brought before the Society the following April.^b If we had no further information regarding them the interval would have been too short to enable us to form an opinion on their permanent results.

A few years ago, however, more evidence transpired regarding these cases, and of one of them in particular. In the second edition of Sir Henry Thompson's work on Stricture,^c he mentions that Mr. Williams told him that he had "recently seen" the patient, and found the urethral orifice perfectly free from contraction. This information must have been imparted by Mr. Williams shortly before his death, so that the result of the operation in that case, at least, must have been tested by a period of several years, and is therefore most conclusive evidence in favour of the operation.

M. Ricord and Weber, as already mentioned, performed an operation similar in principle to that of Colles. Ricord had recourse to

^a Within the last few days J. A. wrote to me saying he can pass a No. 13 bougie.

^b Dublin Medical Press, April 28, 1841. P. 257.

^c The Pathology and Treatment of Stricture of the Urethra and Urinary Fistula. Second Ed. London, 1858. P. 250.

the operation to prevent coarctation of the urethra after amputation of the penis, and Weber performed it for congenital narrowing of the urethral orifice with phymosis.

The following operation was performed by Weber on his patient. The phymosis "having been operated upon, a triangular flap three quarters of an inch in length was cut at the posterior part of the glans, by making with a scissors two divergent incisions, having for a common point of departure the narrowed orifice of the urethra. This flap was then denuded of its epidermis, turned back in such a way as to evert its mucous surface, and fixed in this position by three points of suture, probably after having raised the epidermis from the corresponding portion of penis upon which the flap was turned or folded. To prevent the edges of the lateral wounds from uniting at their summit, the mucous membrane was turned towards the external skin, and united to it by a point of suture at each side. Cicatrization was obtained in great part by the first intention. No sound was introduced to prevent the contact of urine with the wound; but they had recourse to a very simple and ingenious proceeding: to micturate, the patient submerged his penis in a vase full of water; in this way the urine was diluted to a degree to be no longer irritating."^a

Colles's operation was at that time (1855) apparently unknown in Paris, if we may judge by the following question by the editor of the *Bulletin*:—"This procedure, does it belong to M. Ricord or to M. Weber? This is a question it is of little consequence for us to resolve; but we do not wish to allow a very simple and ingenious practice to fall into oblivion, and one which is applicable as well to the urethra after amputation of the penis as to congenital narrowing of the meatus."^b

Dr. Charles Benson having put a very pertinent question, bearing upon Colles's operation and amputation of the penis, to Mr. Williams, the evening he brought his cases before the Surgical Society, it may be advisable to reproduce here the description of M. Ricord's operation:—"One of the most unfortunate consequences of amputation of the penis is the increasing contraction of the urethral orifice, the result of the cicatricial process which takes place from the circumference to the centre. The frequency of this accident has led surgeons to the section

^a Bulletin Général de Thérapeutique. Tome 49. Paris, 1855. P. 333.

^b Ibid.

of the urethra at the moment of the operation; no operation practised hitherto has secured the patient from this inconvenience. According to M. Calvo, M. Ricord has succeeded in filling up this gap, in the invention of the following procedure:—After having practised amputation of the penis with a hot iron, this surgeon removed from the inferior part a V-shaped flap of skin, setting out from the wound. The urethra thus exposed was in its turn divided to an extent equal to that of the wound made in the skin. This section was made with Civiale's urethrotome. The division effected, the lips of the urethral wound were everted in such a way as to be easily united to the cutaneous wound, by means of three points of interrupted suture. The result was an artificial hypospadias. The procedure has succeeded perfectly in the two patients to whom M. Ricord has applied it; it was not necessary to keep sounds in their urethræ; the only precaution taken was micturition in cold water, to avoid the unfavourable influence of the urine upon the lips of the wound, which united by first intention."* It is quite evident that when the above observations were penned, the editor of the *Bulletin* was not aware that during the discussion on Mr. Williams' communication at the Surgical Society's meeting (April, 1841), Dr. Charles Benson, then Professor of the Practice of Medicine in the Royal College of Surgeons, Ireland, inquired if Mr. Colles's operation "was applicable to that form of stricture which occurs after amputation of the penis," and received in reply from Mr. Williams that "he had not seen it tried in such cases, but he could not see any reason why it should not, and it would be even more easy of application to them."

Intimate Mixture of Cancer with Enchondroma.

There could scarcely be a more interesting question in surgical pathology than the correlation of cancer and enchondroma with one another. It has long been known that they may co-exist as separate and well-defined masses in the same tumour. Of this combination, an excellent example occurred in a patient who was admitted to the Adelaide Hospital a few years ago. He was thirty-five years of age, and was operated upon by my colleague, Dr. Albert Walsh, who removed from the right side of the scrotum a large tumour that on section was found to be formed inferiorly of a well-defined encapsuled mass of enchondroma, and superiorly of

* Ibid. Tome 48. 1855. P. 89.

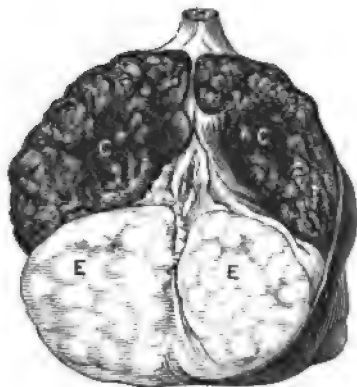


Fig. 11.—Enchondroma and cancer of right testicle. C C—Medullary cancer. E E—Enchondroma, isolated from the cancer by a firm cyst. Reduced from one of the Adelaide Hospital pathological drawings.

most decided medullary cancer (Fig. 11). There was no trace left of the right testicle. The patient recovered, but subsequently died from a large medullary cancer implicating the right kidney. Again medullary cancer has followed upon the removal of an unmingled enchondroma. Not only may enchondroma be mingled with cancer in the primary tumour, and followed by cancer as a secondary disease; but there is evidence on record in favour of the opinion that the cartilaginous tumour, in the same way as cancer is itself capable of infecting the constitution. Those who consider that enchondroma has this infecting influence attribute it to its heteroplastic nature and occasional richness in juices.^a Indeed, Virchow maintains that enchondroma may reach the extreme malignity of cancer, and observes, that if all malignant tumours are to be called *cancer*, this should be called cartilaginous cancer.

The life histories of enchondroma and cancer are sometimes so alike that operation for the removal of an enchondroma may be followed by the appearance of cartilaginous tumours either on or near the site of the ablated tumour, or even in more distant parts, either on the outside of or within the body.

Some interesting examples of the recurrence of the cartilaginous disease after removal of the primary tumour are mentioned by Paget.^b

^a *Pathologie des Tumeurs*, par R. Virchow, Traduit de L'Allemand, par Paul Aronsohn. Tome, Premier. Paris: 1867. P. 498.

^b *Lectures on Surgical Pathology*, Vol. ii., 1st edition. London: 1853. P. 205.

Vascularization of cartilaginous tumours, *telangiectasic enchondroma*, as it is called by Virchow, to such an amount as to cause pulsation, is calculated to lead to great embarrassment in diagnosis. Vascularization and pulsation being also met with in purely cancerous tumours, and in the mixed cancerous and enchondromatous tumour the difficulty in the diagnosis may become still more puzzling.

With regard to the commingling of enchondroma with other structures an accurate pathology requires that the distinction should be made between cases of primitive combination, in which the different types of tissue appear beside one another, although independent of one another up to a certain point, and cases of, according to Virchow, secondary transformation of one tissue into another in such a manner that, either the enchondroma becomes mucous, medullary, or osseous; or else that, *vice versa*, mucous or fibrous tumours cartilagify. This writer explains the appearance of cancer with enchondroma in the following way:—As long as the character of the enchondroma is not changed, the new cells continue to excrete new intercellular substance around them. If this, on the contrary, diminishes, or if it cease completely, as in the cancerous forms, in which the secretion no longer consists only of a certain liquid juice, then there is change in the type of the tissue, or a transition towards a new type, and there is only occasion for a more advanced development of young cells towards the epithelial forms, in order to produce “a cancrioid or a true cancer.”

Mixed or composite tumours, teratomes, are peculiarly fond of developing themselves in the glands; and sometimes in the same organ, beside one another, there may be four, five, or six different tissues, so that the resulting tumour presents a different aspect in different situations. A rather frequent combination is that which takes place particularly with medullary cancer. If the tumour be mixed there are found most often in it nodules or isolated lobules distributed in very small portions and in points very distant from one another.*

In the following case the unassisted eye was unable to discriminate between the cartilaginous and cancer structures which were so intimately commingled in the tumour that it required careful and repeated examination to differentiate their cellular elements from one another.

* Virchow. *Ibid.*, p. 503.

CASE IV.—E. M., aged about thirty-five years, was admitted to the Adelaide Hospital on the 24th August, 1872, to be treated for a smooth-looking tumour seated over the lower portion of the right temporal region and the zygoma, as well as over the greater portion of the superficial boundary of the parotid region (Fig. 12). Measured with the calipers the greatest vertical diameter of the tumour was three and a-half inches; the antero-posterior diameter being two and a-half inches. It was very prominent, and its investing skin was livid in colour, and beset with numbers of minute veins. At the most prominent part of the tumour there was a circular opening in the skin about an inch in diameter, through which a vascular-looking fungus protruded. This fungus emitted a fœtid odour, and bled from slight injury.

The tumour was very movable on the underlying structures, and pulsated softly and synchronously with the arterial pulsations. A varicose vein coursed along the outer boundary of the orbit. There were no enlarged glands in the neighbourhood of the tumour, neither was there any buccal obstruction.

The patient gave the following history of the tumour:—Five years previously she discovered a small lump, about the size of a hazel nut, in front of the right ear, and without discolouration of the skin over it. Increase in size gradually took place, and at the expiration of two years and a-half it changed in colour, began to throb, became more painful, and grew more rapidly. About this time a heavy shutter fell against her forehead and right temple, but she is not certain if the tumour was struck by it. Three months before admission to hospital bleeding to a considerable amount took place from the protruding fungus, and she was thereby much weakened. The bleeding was eventually stopped by local applications, and did not recur, but the tumour has ever since been more painful.

When the patient had been in hospital a few days I examined a small scraping from the surface of the fungus, and found that it contained some large, clear, plump-looking nucleated cells.

Her general health appeared to be good.

9th September.—The patient being fully under the influence of chloroform I removed the tumour, assisted by my colleague, Dr. Barton, and by Dr. Wharton, who kindly lent his assistance in the absence of Dr. Walsh on the Continent. The skin appeared so altered, and was, moreover, so adherent to the tumour, that even if inclined to do so, none of it could be saved. The tumour was

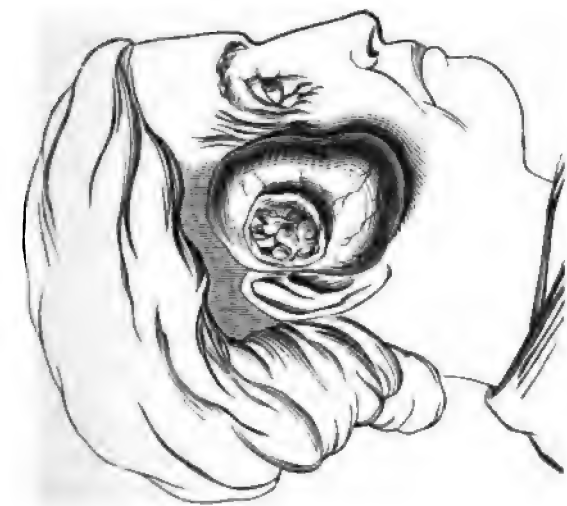
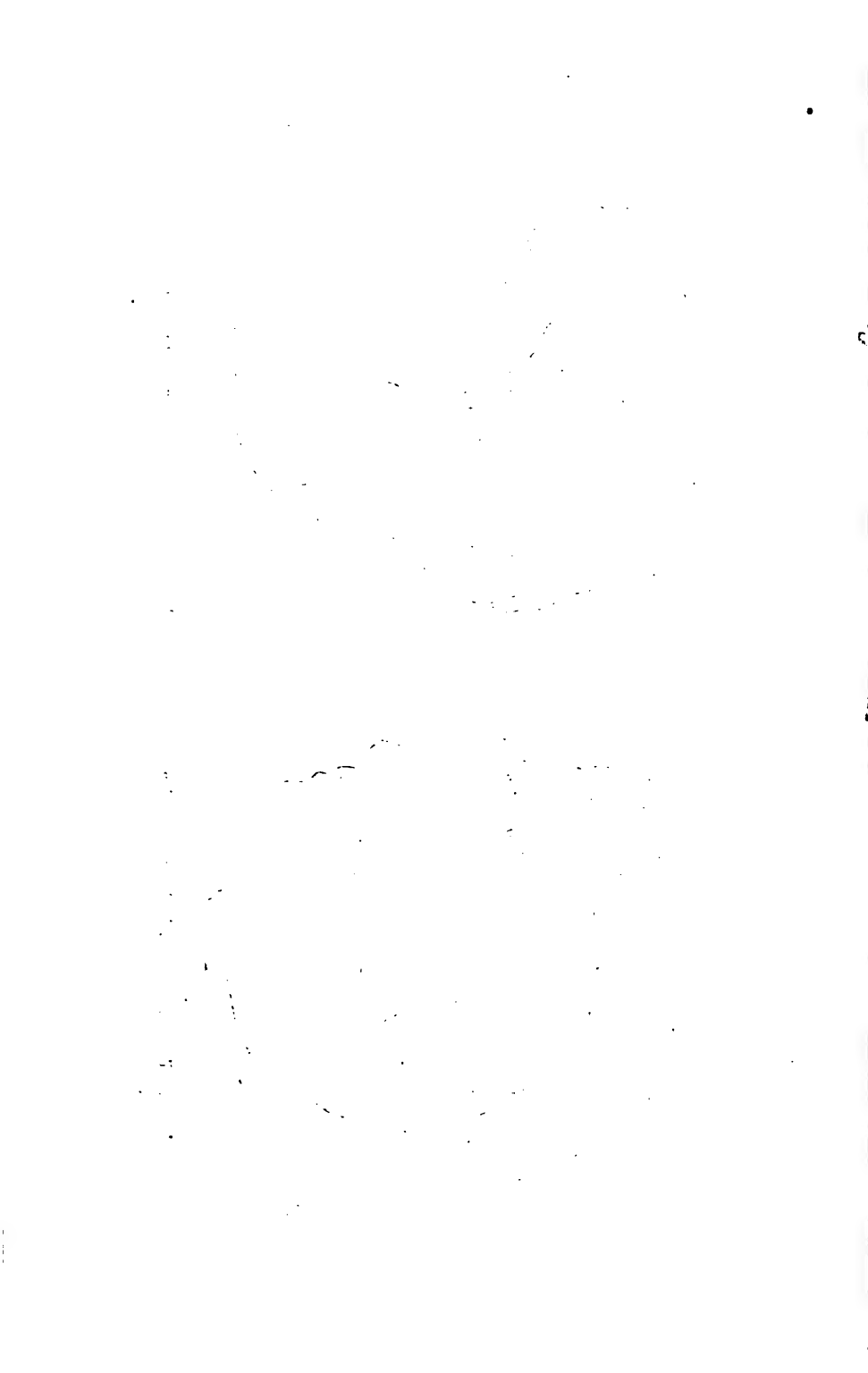
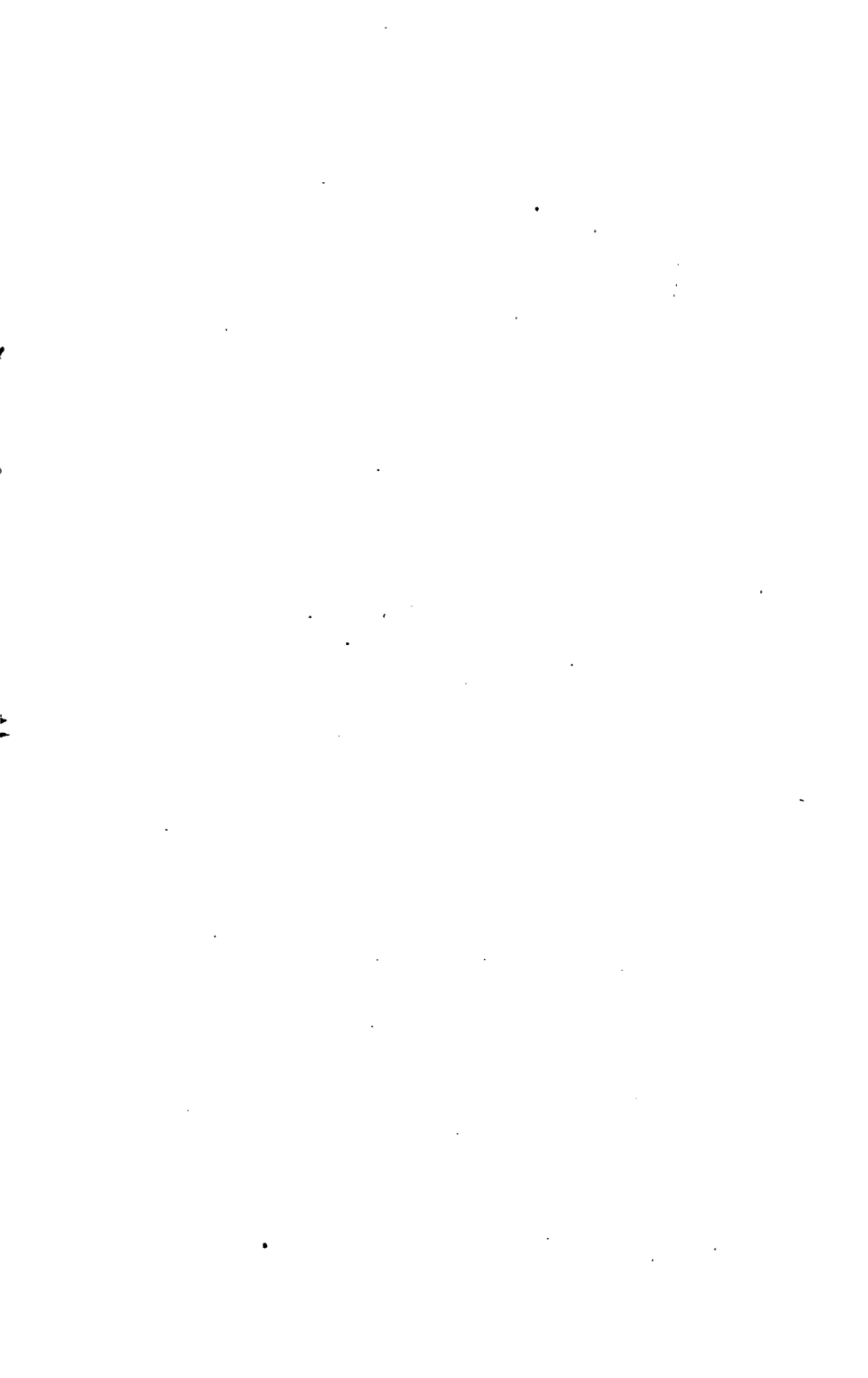


Fig. 12.—The tumour of E. M. before operation.



Fig. 13.—E. M. when the wound was completely healed.





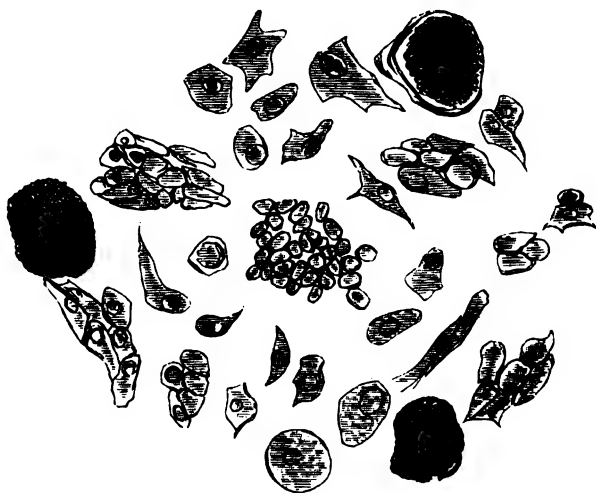


Fig. 14.—The cancer cells from E. M.'s tumour. The large dark bodies are granule cells. One of these being encapsuled with more than one layer of capsule is probably fatty metamorphosed cartilage. Drawn with Nachet's Camera. 230 diameters.

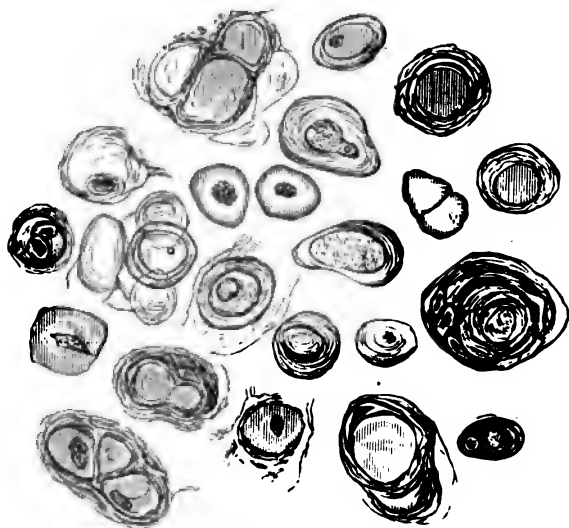


Fig. 15.—The cartilage cells from E. M.'s tumour. Drawn with Nachet's Camera. 230 diameters.

perfectly isolated from the underlying structures by a thin, but firm cyst, and slightly indented the parotid gland. Seven vessels were ligatured, all of them having retracted too much for satisfactory torsion, and, as there was a good deal of oozing, a strong solution of perchloride of iron was applied. Carbolic dressings were subsequently used, and by the end of November the wound had completely healed (Fig. 13).

Pathology of the Tumour.

The tumour was invested by a thin firm capsule, which was intimately adherent to it by the inner surface, and by its external surface to the portion of skin that covered it. The tumour having been laid open by a long incision, the two halves were then fully exposed by tearing, which was effected by very slight traction. The separated surfaces were buff-coloured, the torn portions being granular. They were beset with small openings, resulting from the division of blood vessels, which were very numerous in the tumour.

Some thin sections having been made and stained for microscopic examination, revealed, when examined, what I believe to be an intimate mixture of cancer with enchondroma. For instance, there were numbers of multiform-shaped nucleated cells, numbers of oval and of round clustered nuclei, and many large dark granule cells. One of these having two or more layers of capsule, I am inclined to consider metamorphosed cartilage (Fig. 14). There were also a good many cholesterine plates, which, together with the presence of the granule cells, show that fatty metamorphosis had taken place in parts of the tumour.

Here and there in the different sections examined there were cells that contrasted remarkably with the thin cells seen in Fig. 14. Those cells were separately, or each, embedded, as it were, in masses of cancer cells, and their being encapsulated within one or more layers of capsule, leads me to believe them to be cartilage cells (Fig. 15). The cells of this figure were derived from different sections of the tumour, and were placed in one figure for convenience of illustration.

The history of this tumour is not like the history of a tumour primarily cancerous, having been discovered by the patient five years before her admission to hospital. It was then the size of a hazel-nut, and must have been there for some time previously.

Presuming that my view of the nature of this tumour be correct, its change from indolence to a more rapid development points to the period when the cancerous element was superadded to it.

If the cartilaginous elements predominated over the cancer elements in the tumour, I should hesitate to give so positive an opinion regarding its nature, believing that enchondroma alone may be highly vascularized, may ulcerate and fungate, may grow rapidly, and there are reasons for supposing, may infect the system.

ART. II.—*Select Clinical Reports.* By ARTHUR WYNNE FOOT, M.D., Junior Physician to the Meath Hospital.

- I.—CIRCUMSCRIBED PULMONARY GANGRENE FROM HÆMORRHAGIC INFARCTION, THE RESULT OF SUBMERSION; FREQUENT HÆMOPTYSES; RECOVERY.
- II.—CIRCUMSCRIBED PULMONARY GANGRENE FROM HÆMORRHAGIC INFARCTION, THE RESULT OF SUBMERSION; FREQUENT HÆMOPTYSES; RECOVERY.
- III.—EMPHYEMA OF THE LEFT SIDE OPENING SPONTANEOUSLY IN THE LEFT LUMBAR REGION; LUMBO-PLEURAL FISTULA.
- IV.—EMPHYEMA OF THE LEFT SIDE PASSING BEHIND THE INTERNAL ARCHED LIGAMENT INTO THE LEFT PSOAS MUSCLE, AND THENCE INTO THE VERTEBRAL CANAL; DEPOSITION OF LYMPH AND PUS UPON THE POSTERIOR SURFACE OF THE THECA VERTEBRALIS.

CASE I.—*Circumscribed Pulmonary Gangrene from Hæmorrhagic Infarction; frequent Hæmoptyses, the result of Submersion; Recovery.*

A CATTLE dealer, overtaken in his cups, was waylaid by some drovers who owed him a grudge; having robbed him of £200, they dropped him into the Liffey, whence he was rescued when half drowned, and carried to an hospital, where he lay a long time in his wet clothes while the contents of his stomach were being removed with a pump. He went home the next day and put himself into the hands of his private medical attendant, who considered him suffering from pneumonia resulting from the wetting; and in about

three weeks after the immersion—2nd September, 1872—he was admitted into the Meath Hospital.

He was a tall, strongly-built man, aged thirty-five, of a sound constitution, and very reticent about the circumstances which led to his illness, which were ascertained from quite another quarter. He had been seized with pain in the chest and rigors the day after the immersion, and in about a fortnight afterwards the fœtor on his breath and expectoration were observed. This “stench” annoyed himself, and used to make his children sick when they came into the room. He presented a pale, haggard, and exhausted appearance; referred the seat of his complaint to the antero-inferior part of the left side, over the eighth and ninth ribs, just external to the mammary line, where, until two or three days before admission, he had had severe pain, though he had none there at present. He lay on the left side, because it gave him relief to “squeeze that side,” otherwise he used to lie, by choice, on the right side. His cough was almost constant, and expectoration profuse, consisting of a brownish-green substratum, and much watery mucus surmounted by a greenish-yellow froth. The odour of the sputa was sweetish and fœcal. He was drenched with constant perspirations, usually brought on by coughing. The only physical signs that could be detected, further than some bronchial râles, were in the left infra-scapular region, where there was a loose muco-crepitant râle audible over a limited space, and surrounded by a belt of indurated lung, in which tubular respiration was heard. There was a striking absence of fever, both pulse and temperature were low, the respirations were 30, the bowels loose. He was put on a liberal allowance of stimulants, and ordered thirty-grain doses of sulpho-carbolate of sodium every third hour. After the fourth dose he ceased to perceive the fœtor himself, but that of the spit-cup continued to be very marked. The pain in the antero-lateral part of his chest, which was evidently of a pleuritic character, returned on the evening of the 4th, caught his breath, and prevented him from coughing; it was relieved by poulticing. On the 9th he was ordered an ounce of the *mistura creosoti* every second hour, as the sulpho-carbolate of sodium did not appear to have been very efficacious. In the meantime it had been observed that the fœces were specially and peculiarly offensive, whereas there was no smell observable from the skin or the perspirations, which continued to be profuse. His appetite was good; he was able to eat meat and eggs, and had six ounces of wine, two ounces of brandy, and extra milk

daily. He pointed out the fact that he perceived the fœtor most when an eructation of wind preceded a fit of coughing. He was so weak that he could not stand steady when he had occasion to get out of bed. The fœtor seemed to diminish under the use of the creosote mixture; but during the progress of the case it was ascertained that the improvements in this respect seemed to be attributable rather to variations in the elimination of the products of putrefaction than to the action of medicines, at least I could not avoid arriving at that conclusion from close observation. This fact has been embodied by Dr. Stokes in his third conclusion on the subject of gangrene of the lung—"That in the progress of a case we may observe the septic action singularly variable." ^a On the 11th the pleuritic stitch had returned, but he said the "smell was teetotally gone," except that "sometimes a real stenching breath comes up." At this time he had taken 24 ounces of creosote mixture; the following night it became as bad as ever, and the spit-cup had a number of greenish pellets floating in a dirty brownish mucus. The following night he slept well for seven hours, had very little cough, but was "in a teem of perspiration," and felt his breath quite sweet, though the fœtor was strong on the sputa, such were the variations in the intensity of the odour. The creosote was omitted now, and he was given 15m. ter die of liq. ferri perchloridi, principally on account of the excessive perspirations, which had not been lessened by full doses of quinine. The day after this change was made he reported that the past night was the worst he had had; that the perspirations "are murdering him." Towards morning he coughed up two putrid collections of gangrenous *debris*, which he called "boils," probably from some resemblance which they bore to the core of a boil. These were intensely foetid, reddish-brown in colour, and described as having a "slimy taste, as bitter as gall;" he felt much relieved after their discharge. On the 16th he remarked that his urine was foetid, and said that "the smell of his water was the same as used to come up his neck." The fœtor of the spit-cup was very bad, though he did not complain of it annoying himself. On the 17th, ten ounces of his urine was examined; no fœtor could be detected on it when cold. It was of a deep, red-brown colour, sp. gr. 1025, very acid, and contained neither sugar nor albumen. Inhalations of the vapour of turpentine had been commenced the day he drew attention to the smell of his urine, and this, possibly,

^a Dublin Medical Journal, Vol. ix., p. 20.

might have acted as an alterative. At noon on the 17th he had a slight hæmoptysis, followed by the expectoration of a reddish ichor, which changed the colour of the usually greenish-brown sputa. The fœtor of this expectoration was extreme and odious. He felt the same kind of relief after the hæmoptysis as he had on the previous occasion after coughing up the "boils." This first hæmoptysis occurred five weeks after the commencement of his illness. The difference in degree of fœtor of the tidal and reserved air was very marked and frequently tested, the air coughed out being much more foul than that simply breathed out: the simply expired air was often almost inodorous, when that coughed out was very offensive. He was now (17th September) put on 10m. doses of liq. sodæ chlorinatæ three times a day, and this was the only medicine which he considered to be of decided benefit to him. The inhalations of turpentine vapour, which were taken twice a day, had the effect of making him sleep, for which purpose he declared it to be "the grandest thing in the world." 19th September, coughed up another "boil," sputa tinged with blood and very fœtid. From this date to the 30th the fœtor diminished, first of the breath, then of the sputa; and although there was hæmoptysis almost every second day, he began to leave his bed. The pleuritic pain occasionally came and went. There was no alteration in the physical signs except that the isolated dulness in the left infra-scapular region gradually disappeared. He left hospital on the 30th September, rather sooner than was prudent, since on that day the exertion of coming up stairs had induced dyspnoea and bloody expectoration. He came subsequently to report himself. On the 4th October he coughed up two more fœtid "boils," and he observed that his family complained of the smell, although he did not perceive it himself. He was given a number of pills of Chian turpentine, with 2m. of creosote in each, one or two to be taken during the day. He was seen again on various occasions, and there had been no return of hæmoptysis or of "boils;" the cough, expectoration, and fœtor had very much diminished. When last seen on 28th October he was looking very well, felt quite strong, and considered himself to be fully recovered. During the time he was in hospital he was singularly free from fever, his temperature being frequently subnormal. Of twenty-eight observations made on his pulse, the average rate was 70·39, its highest being 99, and lowest 62. Of twenty-six observations made on the temperature, the average was 96·8° Fahr., the extremes being 100° Fahr. and 95·8° Fahr.

and of twenty-six observations on his respirations, the average rate was 26.4 in the minute, ranging from 34 to 22. He was not an habitually intemperate man; he was naturally endowed with a good constitution; he had been very carefully attended to before he came into hospital; his disease appears to have been of limited extent; and it is very probable that this combination of favourable circumstances materially promoted his recovery. I had no doubt whatever that its nature was such as is stated at the head of the article.

CASE II.—Circumscribed Pulmonary Gangrene from Hæmorrhagic Infarction, the result of Submersion; frequent Hæmoptyses; Recovery.

The subject of this case was a grainer and decorator, aged thirty-seven; he had saved five men and one woman from being drowned, and had obtained both the silver and bronze medals as well as the certificate of the Royal Humane Society. On Saturday, 20th July, 1872, he rescued a gentleman from the canal at Blacquiere Bridge with much difficulty, as he was a heavy man, and he had to dive for him. He was three hours in his wet clothes after coming out of the water; four days afterwards he had rigors followed by a pain in his left side which caught his breath; he gave over work for a week, and afterwards worked off and on at intervals as he felt able, till the 30th of August, on which day he was employed in the house of a medical man, who, hearing his peculiarly hard, long cough, spoke to him about it, and upon hearing his history sent him to me with the view of his being taken into hospital, which was done at once. He was under the middle stature, wiry in appearance, courage stamped on his face; he said that, sick as he was now, he would go into the water if he saw anyone drowning; had always been very temperate, and had had "a good chest" till the present attack. He was admitted into the Meath Hospital, 30th August, 1872, six weeks all but one day after his immersion, suffering from an incessant hard loud cough, with profuse expectoration, owing to which he had hardly had any sleep for the last fortnight. For the last ten days before admission he had also been annoyed by "gas coming off his chest;" this smell was very perceptible when he coughed, and came out suddenly in great bursts of stench when he had a severe fit of coughing; it had the odour of intestinal flatus, while the sputa, which were in part

frothy, watery, and muco-purulent, and in colour greenish-brown, had a sweetish fæcal smell. He suffered greatly from night sweats, had pain in the lower part of the left side of his chest, and could not lie on his left side; there were no physical signs, other than those of bronchitis, with the exception of a special local muco-crepitant râle below the left nipple. He was put on the most nourishing diet with stimulants, and ordered 30 gr. doses every third hour of sulphocarbonate of sodium; this was not found efficacious, and in two days he was put on an ounce of *mistura creosoti* every second hour, and directed to inhale the vapour of turpentine. He could not continue the turpentine inhalations, as they caused nausea. The first signs of hæmoptysis appeared on the 8th, just seven weeks after the commencement of his illness; on the 10th the sputa were of a homogeneous brownish-red colour, exactly like anchovy sauce; a rise in the temperature excited apprehensions of more hæmorrhage, and this occurred to a considerable extent during the day whenever he coughed, and continued till 6 p.m.; the fætor of the bloody sputa was very marked. Up to the 13th there were traces of blood in the expectoration. To check this intercurrent hæmoptysis he had a mixture of ergot of rye, gallic acid, dilute sulphuric acid, and infusion of *krameria*; the *mistura creosoti*, which had not had more influence over the fætor than the sulphocarbonate of sodium, had been discontinued. On the 13th, the hæmoptysis having ceased, he was ordered the following:—Liquified carbolic acid 20m., dilute acetic acid, tinct. of opium, chloric ether, of each ʒi. water to 8 oz.; an ounce three times a day. Next day there was a marked fall in the pulse and temperature. He liked this mixture very much, and observed that a confusion and lightness in his head had disappeared. Anæmia from the hæmoptysis gave his face a sallow hue, but the eyes were quite free from any icteric tint. The quantity of expectoration in the twenty-four hours remained copious, varying from five to nine ounces; the fætor also showed great variations, and he was himself subject to very rapid changes of what, for want of a better term, must be called feeling better and worse. He was able to be up for a short time on the 18th; next day he was ordered bark with chlorate of potash and dilute muriatic acid; hæmoptysis returned on the evening of that day after the exertion of vomiting, and continued in a moderate degree for twenty-four hours. On the 23rd he noticed the sputa "very gassy." During the night of the 26th he felt as if "something

had given way inside him," and he expectorated a quantity of a brownish-pink fluid foetid matter, like what goes by the name of "grumous pus;" he felt much relieved in the morning, said his appetite had returned, and asked for more food, which was allowed him. He was ordered on the 28th 10 m. liq. sod. chlor. ter die, and often said this medicine did him more good than any of the others. It is to be noticed that the expectoration now became changed in character, assuming a much more puriform quality than it had hitherto presented, and becoming of a yellowish colour instead of the greenish-brown it had generally been when not bloody or coloured by the medicines; the sputa becoming yellow is noticed as a favourable symptom by Niemeyer. On the 30th he was down stairs and out in the grounds; according as he got better he first ceased to perceive the "gassy smell" when he coughed, then it disappeared from the expired air, first from the tidal (breathed out), afterwards from the reserved (coughed out) air. The sputa retained the foetor longest. On the 15th October there was still foetor off the sputa, which were copious, again greenish-grey and dirty white, with much turbid mucus and froth, and the muco-crepitant râle continued audible over a limited space in the left infra-mammary region; this he could hear himself, and he referred to it as the source of the constant expectoration. He had no pain anywhere, nor has he had for a long time any except a "general soreness from coughing." The foetor having been much subdued, full doses of tannic acid were ordered with the view of diminishing the secretion of the irritated bronchial tubes, but this medicine produced salivation, and he asked for the chlorinated soda, for which he had acquired a great liking; creosote pills had proved ineffectual. On the evening and night of the 19th there was another recurrence of bloody and foetid expectoration, preceded by uncomfortable sensations, and followed by relief, the tint and aspect of the sputa becoming changed for some time subsequently. On this occasion, on careful auscultation about the left infra-mammary region, short sub-tubular respiration with rhonchi and clicks—indicative of small cavities—were heard for some time; late in the case he had a "burning pain" in this region, which after a while yielded to blisters. Repeated returns of slight hæmoptyses, always attended with accessions of foetid sputa and "gassy cough," occurred, but they lessened almost imperceptibly, and he got better by slow degrees, till at last he was able to go to the Convalescent Home on the 23rd November—

twelve weeks after his admission into hospital, and eighteen weeks after the immersion which brought on the illness. During the last month in hospital he gained several lbs. in weight. On the 16th of December, he was free from cough, fœtor, and expectoration, and considered himself to have quite recovered. An analysis of the "Clinical Chart of Temperature," &c., shows that of 97 observations on the temperature, the highest recorded was 103·5° Fahr. (evening); the lowest 96·9° Fahr.; the mean was 99·6° Fahr. The number of observations made after the severity of the disease had subsided, has reduced the average height of the temperature; it was on twenty-six occasions at or above 101° Fahr. during his stay in hospital. The mean of 55 observations made during the first twenty-eight days after his admission is 100·5°; it was subject to great variations, and a rise in temperature frequently gave warning of impending hæmoptysis. Of 96 observations on the pulse, the maximum rate per minute was 110, the minimum 72, and the mean 87·04. Of 94 observations on the respirations, the maximum rate per minute was 40, minimum 20, the mean 26·28.

It would be unjust, in publishing these cases, to omit mentioning the sustained attention paid to them by the practising pupil, Mr. De Renzi, in whose charge they both were, and I am the more prompted to do so from a knowledge that they were not of a nature calculated to be the most interesting to a student, on account of their tedious and rather monotonous length, and from a belief that one of the best means of promoting clinical study is to show the most marked approbation of all who devote themselves to its toil with assiduous diligence. The two cases above related were curiously alike in cause and course; the physical signs, singularly obscure as they were, indicated the left lung as the seat of disease in each; they had the same profuse sweatings in the early, hæmoptyses in the middle, and diarrhœa in the later stages; the same prostration of strength quite out of proportion to the physical signs; the same variability of symptoms, one day better, another day worse; both were singularly free from very high fever, one had hardly any pyrexia; there was apparently little that was characteristic about them but the fœtor; they walked about much of the time that the disease was going on; they conformed exactly to the behaviour of a group of cases described by Niemeyer:*

* Pract. Med. Vol. i., p. 125.

"Others bear this serious disorder wonderfully well; their general condition is scarcely changed; they walk about, are without fever, and the disease goes on for weeks. In these cases hæmorrhage may arise at a late period."

The view which I took of these cases was that they were of a similar nature, and that they were cases of circumscribed gangrene of the lungs, representing the typical issue of hæmorrhagic infarctions. They belong to the class of curable gangrenes of the lung, on some forms of which Lasègue^a has written. The histories of both the cases appear to me to warrant the opinion that extreme congestion of the lungs during submersion produced pulmonary apoplexy and extravasation of blood into the lung tissue. This cause of putrefactive gangrene of the lung from decomposing blood, so fully worked out at present, was many years ago advanced by Dr. Law, in his paper read 7th September, 1829, on "Cases of Putrefactive Disorganization of the Lungs,"^b and Professor Robert Smith^c and Mr. O'Ferrall^d speak of this condition as first described by him, while Lasègue is inclined to attribute it to M. Genest,^e but does not speak with certainty on the subject, as he only says that Genest was *probably one of the first* to insist upon this mode of production of gangrene; however, the date of his communication is several years subsequent to that of Dr. Law. Trousseau^f bears his testimony towards the frequency of hæmorrhagic nuclei as the starting points of circumscribed gangrene of the lungs. The pulpy plugs and crumbling detritus expectorated from time to time, the frequent hæmoptyses, the pathognomonic fœtor, the variability of all these symptoms, the absence of physical signs, are prominent features of this form of gangrene. Woillez^g puts pulmonary apoplexy first on the list of special causes of this condition—circumscribed gangrene of the lungs, and Rindfleisch^h points out that when the air-cells are filled with coagulated blood, circulation entirely ceases in the part, and as no body under favourable conditions putrifies so easily as coagulated blood, gangrene of necessity occurs. The extension into the surrounding

^a Arch. Gén. de Méd., 1857, t. 2, p. 26.

^b Trans. Med. Assoc. Coll. Phys., n. s., Vol. i., p. 89.

^c Proc. Path. Soc. Dubl., 9th May, 1840.

^d Proc. Path. Soc. Dubl., 16th May, 1840.

^e Gaz. Méd., 1836.

^f Clin. Med., Vol. iii., p. 174. Syd. Soc. Trans.

^g Dict. de Diag. Méd., p. 431.

^h Pathol. Histol., Am. ed., pp. 429, 439.

healthy tissue of gangrene from decomposing blood clot is prevented by a salutary inflammation of the adjacent lobules which limits and circumscribes to a certain extent, in favourable cases, the gangrenous depôt. When these cases came under notice, the pathognomonic sign of gangrene, the peculiar fœtor, was present, but there can be little doubt that until that event occurred they presented the undefined character which cases of this form of disease so notoriously do, and which had led Niemeyer^a to remark that circumscribed gangrene from hæmorrhagic infarction cannot be diagnosed until the gangrenous discharge enters a bronchus and is ejected. The histories of the cases and the physical examination excluded at once many other possible causes of fœtid breath and expectoration. There were no bronchiectatic dilatations in the ampullæ of which the secretions tarry till they decompose, nor any evidence of vomicæ, nor of metastasis of fœtid matter through the lungs from the pleura. The bronchitis, which was more or less present all through, was always regarded as the result of the irritation of the putrid ichor and of the constant coughing, and the character of the sputa prevented them from being for a moment classed among the cases of fœtid bronchorrhœa which can exist quite independently of any gangrenous action, at least any discoverable after death. In further studying the etiology of the gangrene in these cases, the question of its possible origin from pneumonia had to be discussed, rather for the sake of clinical instruction than from the existence of any probability of such a cause; but this view was not tenable, inasmuch as the patients did not relate the symptoms of pneumonia, but rather those of pleurisy, as having been the results of the submersions. The rarity also of gangrene as a result of pneumonia is generally received, and is shown by Grisolle,^b who in 305 pneumonias did not find one case so terminating. Rindfleisch accounts for this fact by the difference in the nature of the exudation in pneumonia. Rokitsanski^c also supports this view, observing that gangrene of the lungs is an affection which, "as Lænnec very correctly remarks, must not be regarded as the result of an excessively acute inflammation." Trousseau^d did not meet with a single case of gangrene of the lung following pure pneumonia.

^a Pract. Medicine, Vol. i., p. 205.

^b Traité de la Pneumonie, p. 345.

^c Path. Anat., Vol. iv., p. 94.

^d Clin. Med., Vol. iii., p. 71.

A hopeful prognosis was made in consequence of the diagnosis of the cause of the gangrene, and on account of the kind of constitutions which the disease was at work on. Many who saw them looked doubtfully when they smelt the disease, having, perhaps, formed their opinions from the diffuse form, or from the results of pulmonary gangrene as observed under the conditions of insanity, inanition, alcoholic excess, or in the course of debilitating diseases such as fevers. There is, however, much comfort to be derived from an acquaintance with the experience of the authorities on this subject. Dr. Stokes^a eighteenth conclusion, in the paper before alluded to, is "that the disease, though always of a formidable character, is not necessarily fatal." Cruveilhier^b observes that the curability of gangrene of the lung is based upon clinical observations and pathological facts. Let no one imagine, says Lebert,^c that pulmonary gangrene is necessarily mortal; anatomically one may study the mode of cure of this gangrene by the elimination and expectoration of the gangrenous substance, and then by the organization, retraction and cicatrization of the abscess which remains after the expulsion of the gangrenous parts. He, however, had only observed this fortunate termination when the patients had neither been living under bad hygienic conditions, nor subject to severe or debilitating chronic diseases. The curability of gangrene of the lung has been as well observed in this country, and as ably reported, as elsewhere; for example, we have the experience of Professor O'Connor, who for several years conducted the largest workhouse hospital in Ireland, and that during a period of famine, given in the Transactions of the Cork Medical and Surgical Society.^d Two of the most marked cases he ever had to treat recovered perfectly. One of them had afterwards to undergo a very searching examination for the purpose of effecting an insurance on his life, and the other had been under his observation for a period of five years since his illness, and continued to enjoy perfect health. A third case died of another affection more than twelve months after Dr. O'Connor had diagnosed the existence of gangrenous eschars in his right lung, and he had an opportunity of exhibiting this lung to the society, containing two cavities with walls perfectly organized, and evidently prepared to cicatrize if the patient had lived much longer.

^a *Dubl. Quart. Jour.*, Vol ix., p. 1.

^b *Anat. Pathol.*, 1862, t. iv., p. 321.

^c *Traité d'Anat. Pathologique*, tome i., p. 659. ^d *Dub. Quar. Jour.*, Vol. xxv., p. 449.

Several observations were made upon the sputa of the two cases above reported. Flies were attracted to them in great numbers, but seemed to perish soon in the mephitic vapour. There were not many flies in the wards during the cold and wet September of this year, but almost all that were were trapped in the spit-cups of these patients. Instinct may have lured them there for the purpose of oviposition, as the flesh-flies are led to the carrion flower (*Stapelia hirsuta*) at the Cape of Good Hope. Whether this were the case or not, it seemed a strong evidence that they appreciated an odour of decay; they did not frequent the spit-cups of other patients in the same wards. There was no sugar in these expectorations, as it was looked for and found absent. They also hovered about the patients themselves, and recalled to mind the case mentioned by Dr. Stokes,* of the gentleman with an empyema discharging through the lung, who was advised in consultation to take the air in an open carriage, but who, after proceeding a short distance from his house, was literally driven back by the swarms of flies which, attracted by the odour, settled in myriads on his person and the carriage. The smell of the expectorations of the two patients was precisely similar, very variable in degree, always worse after hæmoptyses, and when any of the "stinking pellets" or "boils" had been coughed up. Very commonly the smell was of that kind described as a mixture of hawthorn or of apple blossoms and fæces. I could not find any elastic tissue in the substance of the pulpy plugs, although I was sufficiently acquainted with its appearance, from the examination of phthisical sputa, nor the crystalized fats occasionally found with the microscope in the sputa of these cases. There was always a greenish or brownish tint on the expectoration; it had a bitter taste while it was passing through the patients' mouths. The smell did not ever sicken the men themselves, although it did others; the daily quantity of expectoration was frequently measured, and found very variable. There was very little effect upon the smell observed from the medicines employed until the chlorinated soda was used. Immediately after the commencement of a new medicine an improvement for a short time used to be observed, but it did not last. Sulpho-carbolate of sodium, creosote mixture, carbolic acid, turpentine, chlorate of potash, muriatic acid, and bark; tincture of perchloride of iron; quinine in full doses (9 grs. daily), had little effect. The x. m. of liq. sod. chlor., in an ounce of water, ter die, had the most effect

* *Dubl. Quart. Jour.*, Vol. ix., p. 18.

upon the fœtor, but it was not employed till late in the cases, when the gangrenous detritus had probably been nearly all discharged. Chlorine, in the form of the chloride of lime or soda, combined with opium, has many years ago been recommended by Dr. Stokes,^a and in one of the Clinical Reports by Dr. Graves and Dr. Stokes,^b a pill of three grains of chloride of lime and one of opium, given three times a day, was followed by rapid and marked amendment of the fœtor; the quantity of the chloride was afterwards increased to twelve grains daily. The inhalation of turpentine, originally proposed by Skoda for the fœtor arising from the secretions imprisoned in the dilated tubes in bronchiectasis, appears to be practically confined to the class of cases for which it was originally intended. The application of antiseptics, in the form of sprays, as recommended by Trousseau, was not employed in these cases. Indeed it is difficult to imagine how the cause of the fœtor could be reached in circumscribed gangrene, arising from decomposing blood clot, placed as it is out of reach of local applications, possessed of no organization, and cut off from the circulation by a zone of pneumonic consolidation. It seems as if nature must be trusted to effect the cure in her own way, however disagreeable it may be, by eliminating the decomposing matter through the bronchial tubes, and it also appears most rational to support the patient's strength, and to supply as much fresh pure air as possible. Of course the more successful the attempts at deodorization the more comfortable, will be the position of the patient and his attendants, but I think that in the cases to which I have alluded, the deodorization is only to be effected outside the body, and that nature will not allow of any interference with the doom of a tissue which she asserts her right to expel in her own fashion.

CASE III.—*Empyema of the Left Side, pointing in the Left Lumbar Region; Recovery with Fistulous Opening.*

A dairy boy, sixteen years of age, was admitted into the Meath Hospital 3rd October, 1871, and was put under the care of the late Mr. Dudley Doran. He had been suffering for six weeks before admission from inflammation of the left pleura, the result of a wetting. When admitted he had a good deal of dyspnœa, but he complained more of "a soreness" in his side than of any other

^a *Dis. of Chest*, p. 359.

^b *Dubl. Hosp. Reports*, Vol. v., p. 63.

symptom; he said that this "soreness" prevented him from running, and made him hardly able to walk. He appeared to be recovering from the effects of the recent pleurisy, as the left side of the chest was manifestly becoming deformed from atmospheric pressure. On a level with the nipple line, the left side was a quarter of an inch smaller in measurement than the right, while lower down, on a level with the tenth rib, the left side of the chest was a quarter of an inch larger than the right; there was an obvious fulness in the lower part of the left chest; the angles of the upper left ribs were flattened, the left shoulder lower than the right, and the lateral curvature of the spine to the right was increased. The physical signs of fluid in the lower part of the left chest were well-marked. At the base of the heart and in the carotid arteries there was a very distinct systolic blowing murmur, none in the veins; the action of the heart was much increased when he sat up, the apex beat was on a level with and to the outside of the left nipple; he lay on the left side because "it was not so comfortable to lie on the other." He remained for four weeks in hospital, and all that time his temperature was high, his pulse rapid, and he frequently perspired profusely. His complaint was always of pain or of soreness, usually referred to the lowest part of the left side, somewhere in the lumbar muscles below the twelfth rib; sometimes the pain was felt from the posterior superior spine of the crest of the left ilium passing down the left leg; at other times it was felt in the left knee; sometimes he could not extend the left leg as well as the other one. After he left hospital, which he did because he was impatient at not getting well more quickly, he remained under observation; at Christmas, 1871, the empyema pointed two inches below the twelfth rib, close to the left side of the spine, opened soon after under the use of poultices, and discharged a quantity of pus, which for a long time afterwards continued to come away when he coughed, and continues occasionally to do so still (Christmas, 1872), although he is as well able to go about his work as ever. The deformity of the chest occurred very early in this case, and would perhaps have suggested its being an example of empyema with a contracted side from the commencement of the disease, such as Dr. Stokes^a has alluded to, but that in those very peculiar cases the disease arose from injury. The deformity has since been to a great degree recovered from, as is often observed in the juvenile thorax,

the framework of the chest and the contained organs exhibiting in the young powers of accommodation not retained by the adult. There are many cases on record of empyema pointing in the lumbar region, and presenting some of the characters of lumbar abscess; there is no doubt that the matter was engaged in making its fistulous track at the time the strange irritation in the extensor muscles of the thigh (psoas and iliacus) was going on.

CASE IV.—Empyema of the left Chest, passing downwards behind the left internal arched ligament into the left Psoas Muscle, and thence into the Vertebral Canal; Deposition of Lymph or Pus upon the posterior surface of the Theca Vertebralis; Encysted Diaphragmatic Empyema on the Right Side.

A thin, pale, white-haired, wiry little boy, ten years of age, was admitted into the Meath Hospital 2nd September, 1872, and put under the special care of Mr. (now Dr.) Henry Mallins, one of the clinical clerks. He was one of nine children, of very poor parents, and procured his subsistence about the roads in a very precarious manner; but, though apparently of a most delicate frame, he proved himself to be possessed of an almost reptilian tenacity of life; his quaint old-fashioned manner made him a great favourite, and secured him an extra allowance of sympathy. All that could be gathered of his previous history was that he had been well up to the 26th August—nine days before admission; and that he then got a pain in his left side which obliged him to stay quiet. The 1st September his mother discovered, on lifting him to settle his “bed,” that he could not stand, and the next day he was admitted to hospital. There it was ascertained that he had lost the use of both legs, and could not feel a pinch in either of the lower extremities; that there was stillicidium urinæ, from paralysis of the bladder, and also incontinence of fæces; there was a sloughing bed-sore over the right trochanter, and a dark patch over the left one; the abdomen was very tympanitic, and the superficial veins distended; the left side of the chest was quite dull, a loud pleural creak was audible over the lower antero-lateral region of right chest, respiration was short and rapid, and principally abdominal; he had a great deal of cough, but swallowed the expectoration; the pulse was 110, the apex beat of the heart was more to the left than usual; he made no complaint of pain, was quite clear in his mind, and very intelligent; he eat and drank heartily. On the 4th September

the bladder, which had required the use of the catheter, became very much distended, forming an oval tumour whose consistence and dulness contrasted strongly with the surrounding tympanitis; 30 oz. of high-coloured urine were drawn off. The bladder and the bed-sores were carefully attended to, and the next symptom that appeared was on the 7th, when great general hyperæsthesia, but especially of the abdomen, was observed; he could not bear to be turned or touched; he complained of pain in the back; the friction in the right side of the chest had ceased, and loud puerile respiration was audible over the right lung; diarrhœa, which proved very troublesome on account of the paralysis of the rectum, was checked by the following mixture:—

Ext. hæmatoxyli., ʒij.
 Tr. catechu, ʒij.
 Spt. chlorof., ʒij.
 Dec. hæmatox. ad. ʒvüj., s. ʒss. ʒiis. horis.

This medicine at once communicated a brilliant vermilion colour to the urine, which was alkaline, sp. gr. 1018, very ammoniacal, and deposited an abundance of phosphates. This logwood-coloured urine afforded an opportunity to the class of comparing the difference between the spectra of bloody fluids and of solutions of hæmatoxylon. The bright colour persisted for many days after the disuse of the medicine. On the 9th a thick crop of vesicles, filled with a watery transparent fluid, and resembling dew-drops in appearance, appeared over the front and sides of the abdomen. This eruption was that of *Miliaria crystallina*. He had not at all suffered from perspirations; the eruption was neither red nor white (*M. rubra*, *M. alba*), but the vesicles were filled with a perfectly limpid fluid as clear as crystal. An acquaintance with Hebra's views upon the etiology of the *Miliaria crystallina* suggested a clue to the condition of the patient. Hebra^a has adduced proof to show that the *M. crystallina*—which are quite distinguishable from the *M. rubra* and *M. alba*, to which he would confine the term *sudamina*, so loosely used by many—are always the result of a pyæmic process; that this eruption develops itself only as a result of a morbid process of which the conditions and symptoms are those of pyæmia (so far as these are at present known and understood), and, therefore, that the existence of this

^a Dis. of Skin, Syd. Soc., Vol. i., p. 383.

rash indicates the presence of pyæmia. On the 11th he was able to move the muscles of the right thigh and stir the right toes a little, and was ordered *x. min. ter die* of liquor ferri perchloridi. This, however, did not modify the pungent alkalinity of the urine, which struck both eyes and nose most forcibly. The Miliaria crystallina extended over the whole trunk, but was not found anywhere on the extremities. His appetite was very good, he devoured meat and digested it well; he was wakeful, watchful, chatty, patient, and seemed to suffer wonderfully less than one would have imagined. He never referred to his chest. The abdominal tympany slowly increased; when the distension was extreme, poultices gave him ease; there was no ascites. On 16th, the long prepuce, which had been much fretted by the frequent dribbling of the alkaline urine, and the use of the catheter for a fortnight, became œdematous and sore, but the application of oil and some needle pricks kept it from getting worse. On the morning of the 16th the temperature fell from 100·4° Fahr. to 97·4° Fahr., and the pulse from 100 to 76; the succeeding night he kept the ward disturbed with his moans, and appeared moribund on the following day, but held on until the 19th, when he died at 3 a.m., after seventeen days stay in hospital.

An analysis of his "Chart of Temperature, &c.," shows that of thirty-three observations upon the heat of the body, the maximum temperature was 103° Fahr., the minimum 96·9° Fahr., and the mean 100·3° Fahr. Thirty-two observations on his pulse show that it ranged from 120 to 48, and that its average rate was 90·8. Of thirty-one observations on the respiration, the maximum rapidity was 58, the minimum 26, and the mean 44·5.

The body, examined eight hours after death, was thickly covered, as to the trunk, with the *M. crystallina*, of which Hebra observes, that if there is one eruption which remains visible and unchanged after death it is this. The tympanitis had greatly subsided. The rigor mortis was slight everywhere, but particularly feeble in the articulations of the paraplegic limbs. On making deep incisions into the extensor muscles on each side of the spine, preparatory to the use of the rachitome, pus issued from the wounds on each side of the first lumbar vertebra, thick, greyish-green, homogeneous, and inodorous. When the vertebral canal had been laid open, the posterior surface of the theca vertebralis was seen thickly coated with a shaggy deposit of tightly-adherent brownish lymph, which could with difficulty be peeled off in parts with the

handle of a knife. Patches of yellow pus were interspersed here and there among the flakes of lymph, which had many red dots of newly-organized vessels scattered through it. The lymph and purulent deposit covered the theca from the brachial enlargement downwards; above that part the tendinous, glistening sheath was very obvious; below it it was quite concealed from view. A deposit of a kind similar to that outside the membranes of the cord was found mixed with the loose watery fat occupying the angles of the spinous processes removed to expose the cord. The brain and cord were removed in one piece, and it was seen that there was no disease of the walls of the vertebral canal. The anterior surface of the theca was not in the same condition as the posterior; with the exception of minute traces of deposition it presented its usual appearance. The sheath having been carefully slit up posteriorly, its inner surface was found quite natural, smooth, not adherent to the arachnoid, nor more vascular than usual; neither was the arachnoid membrane dull, opaque, thickened, or adherent, nor was there effusion beneath it. The pia mater was quite unaffected, and the cord itself was unaltered in shape, colour, and consistence. The cranium, brain, and membranes presented no anomaly.

In the chest the right lung was healthy, with the exception of some emphysematous bullæ at its apex, and free from adhesions, except in connexion with its lower lobe, where, encysted between the concavity of its base at the right ala of the diaphragm, was a cavity, which would probably have held a hen's egg, whose sides were rough and shaggy with greenish lymph, and from which pus issued when its lateral adhesions were broken down in displacing the lung. The left side of the chest was mostly occupied with thick, inodorous, gruel-coloured, homogeneous pus; the left lung, thickly sheeted with light, yellowish-green lymph, was compressed, condensed, almost black in colour, very much reduced in size, and pressed backwards. The left chestwall was coated with flaky lymph, which, when peeled off, showed the intensely red costal pleura beneath; the mediastinal pleura on this side was covered with granulated exudation. The inner surface of the pericardium was free from disease, and contained 2 oz. of serum. The heart was healthy, its apex strongly drawn to the left, as if the traction power of the repulsed lung had proved stronger than the excentric pressure of the empyema. The diaphragmatic pleura was in the same condition as the costal and pulmonary portions. Behind the left crus of the diaphragm a small passage was clearly seen, which

passed downwards posterior to the internal arched ligament into an abscess in the left psoas muscle. The abscess occupied the upper two-thirds of the muscle, and did not extend below the point where the anterior crural nerve traverses the muscle obliquely. The psoas sheath was intact; the matter was deep in the substance of the vertebral border of the muscle. There was no matter in the right psoas; that which had appeared at the right side of the back must have burrowed round posteriorly under the lumbar fasciæ from left to right. There was no communication between the encysted empyema on the right side, that could be discovered, and the depôt on the right side of the back. When all the viscera had been removed, the bones of the vertebral column were individually examined. No caries was found, nor any appearance of osseous disease discoverable.

In the clinical discussions which have been held with the medical class in the Meath Hospital, on points connected with empyema, various questions have arisen, some of which are of interest to those who are commencing a career of observation. For example, their attention has been directed to the subject of the relative frequency with which the right and left pleura become inflamed. A careful examination of their notes will show most who have kept accurate records of cases of pleural effusion that the left side seems to be more frequently attacked than the right, a predisposition which is perhaps comparable with the liability of the lower part of the right lung to pneumonia, of the left lobe of the liver to cirrhosis, of the left lung to perforation by vomicæ, and of the right lobe of the thyroid gland to enlargement. Another subject of discussion has been why purulent effusions will frequently make their way out of the chest, even by the most devious tracks, while a serous effusion almost never does so. Sir H. Marsh^a mentions one case in which pointing took place, and "a serous fluid tinged with blood escaped," but this is an occurrence of great rarity. Some hold that the empyema necessitatis is the result of a dissolving property, with which pus corpuscles are endowed, and which enables them to absorb tissues with which they are in contact. Virchow^b does not admit that pus possesses this property, but considers the proper explanation of the apparently corrosive action of pus is, that when the pus corpuscles originate from the connective tissue, the proliferation of the tissue inflamed produces

^a *Dubl. Med. Press*, Vol. i., p. 73. 1861. ^b *Cell. Path.*, 3rd ed., 1868, chap. xix.

the softening, and the pus is the result, and not the cause of the softening. The way in which this is stated by Niemeyer^a is doubtless clearer; speaking of pleuritis with purulent effusion he remarks:—"Not only are pus corpuscles generated upon the free surface (epithelial formation of pus), but they are also formed within the tissue of the pleura itself; the latter becomes opaque, and softens, and irregular losses of substance occur; should they be situated upon the costal pleura and penetrate deeply, external perforation of the empyema may take place. In similar manner a penetration of the empyema into the lung, and its discharge, by way of the bronchi, sometimes happens." As it may be objected that it is not a very common event for empyema to evacuate itself spontaneously, it must be borne in mind that there is another source of the pus corpuscles which result from inflammatory action, and that probably the main source, namely, the migrated white cells of the blood, and it is quite possible that empyemata, whose pus was formed of migrated blood corpuscles, would be bounded by much firmer walls, and consequently, less likely to evacuate themselves than those whose pus was derived from the proliferation of the pleural connective tissue, which would be more or less softened and destroyed by the process. The subject of the determining cause and the source of purulent collections in the serous cavities, which may be looked on as "inner spaces of the connective tissue of the body,"^b has an important bearing upon the frequency of empyema after an illness of a depressing kind—its prevalence in young people, and its occurrence under those conditions which are understood by the term "blood-poisoning."

The passage of an empyema through the diaphragm is its rarest mode of exit from the chest. When the thoracic pus does take this direction, the psoas muscles are, from their anatomical conditions, frequently invaded. The course which it appears to me the *dépôt* in the psoas muscle took in the second case of empyema was that it followed some of the earlier branches of the lumbar plexus, and got into the vertebral canal, where they get out. The exudations on the outside of the back of the theca vertebralis do not appear to have originated from that membrane, nor were the signs of inflammation of the sheath of the cord ever present, as pain was singularly absent—the symptoms were rather those of compression than of inflammation. The matter having once insinuated itself

^a Pract. Med., Vol. i., p. 257.

^b Rindfleisch Path. Hist., p. 253.

into the vertebral canal, could slowly spread to the height observed—the brachial enlargement. In a case of psoas abscess reported by Dr. Bright,^a the sudden supervention of fatal symptoms was manifestly due to this cause; a probe could be easily passed from the intervertebral foramina into the adjoining abscess. When an empyema perforates the diaphragm it is very difficult to say what course it will take. In the remarkable case lately published by Dr. Hayden,^b an empyema of the right side passed through the diaphragm, pointed in the right hypochondrium, and, on incision, yielded about three quarts of pus; he quotes a similar case from Dr. Williams, and also Andral's case, where the pus lodged between the spleen and the walls of the abdomen. Krause^c gives two cases of perforation of the diaphragm by empyemata; in one the matter pointed at Poupart's ligament, and was opened as a psoas abscess; in the other the matter burst into the transverse colon by two apertures, through which fæces passed upwards into the right pleural cavity, which had been the seat of the disease. In a case recorded by Mohr,^d there was a perforation of the diaphragm near the vertebral column, and the pus descended behind the peritoneum along the psoas muscle, with abscesses and fistulous channels in the thigh, extending down to the knee.

^a Medical Cases, Vol. ii., p. 138.

^b Dub. Quart. Jour., Feb., 1869, p. 225.

^c Dub. Med. Jour., Vol. xxv., p. 488.

^d Hasee Pathol. Anat., p. 199.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Cancer: its Varieties, their Pathology and Diagnosis. By HENRY ARNOTT, F.R.C.S., Assistant-Surgeon to St. Thomas's Hospital, and Joint Lecturer on Morbid Anatomy to the Medical College. Illustrated by Lithographic and Wood Engravings from Drawings by the Author from Nature. London: J. & A. Churchill, New Burlington-street. 1872. Pp. 86.

MR. ARNOTT'S book is in great part a reproduction of a series of papers on the same subject contributed to the *Medical Times and Gazette*. The medical public will, if we mistake not, be well pleased to have his memoirs published in a separate volume; indeed, the author is justified in saying, as he does in his preface, that his work endeavours to fill a gap in English medical literature upon a subject of the highest interest.

In the volume before us the author has considerably re-modelled his original papers, and dwelt at greater length on some topics. The illustrations are good, and not only well selected, but admirably adapted for one commencing the difficult study of morbid structures under the microscope.

A main feature of the work is the account given of the simplest methods of examining morbid growths microscopically. There is no doubt that many surgeons hesitate to undertake such investigation, under the erroneous impression that they require much time and labour. Hence many valuable points are lost, and accurate observations of an important group of facts (especially important as regards classification) fail to be accumulated.

Mr. Arnott, although now Assistant-Surgeon to St. Thomas's Hospital, was for some years Surgical Pathologist at the Middlesex Hospital, where he enjoyed unusual opportunities of studying the class of diseases of which he writes. His work is clear, simple, and practical in giving directions for examination of specimens. Upon this subject the following extract will show the reader the

comparative facility with which the histological characters of any tumour may be investigated:—

“For this purpose the surgeon must consent to take a little more trouble than is generally bestowed upon such examinations; but if the chromic acid, carmine solution, glycerine, &c., be kept in some convenient place, with one or two small glass shades to keep out dust, the whole process of cutting sections, staining, and mounting need occupy only a few minutes, and involves very little trouble or difficulty—certainly no trouble which is not amply repaid by the more accurate knowledge of structure so gained. The most useful magnifying power for general employment is the $\frac{1}{4}$ -inch objective with a low eye-glass, magnifying together about 220 diameters. The sketches illustrating this book were cut from drawings made on the blocks, of tumour sections so magnified. But an inch or two-inch objective is also very convenient for giving a general view of the structure, and with a double nose-piece or lens-holder carrying both objectives, these powers may be changed at pleasure without any loss of time.

“It would be foreign to my purpose were I to attempt to introduce here any account of the various processes by which tissues may be prepared for the careful examination of their minute structure; but it may be convenient to mention the simplest way in which the practical surgeon may satisfy himself of the structure of a tumour about which he is in doubt.

“The necessary apparatus comprises only (1) a strong solution of chromic acid (say 20 per cent.), (2) a little pure glycerine, (3) a case containing needles for teasing and lifting small bits, fine scissors, and a razor for making thin sections (a Valentin's knife is more convenient for tissues which are not artificially hardened), (4) a few bottles of test fluids—as ether for dissolving away fat, mineral acid for clearing off calcareous particles, acetic acid for rendering nuclei more distinct, &c., (5) a bottle of Beale's carmine staining fluid (which may be readily prepared at home, or by any chemist), and (6) as many glass slides, thin covering slips, and watch glasses as may be desired.

“Bits of the tumour the size of filberts are put to soak in water, to which a pale straw tint has been given by the addition of a little of the chromic acid solution; and if this be changed daily, two or three days will usually suffice to harden the softest tissue. When this is accomplished, very thin slices are taken from one or more of the bits, the razor being moistened with water or spirit, and these are either mounted at once, or set aside to soak for a few hours in a watch glass containing some drops of staining fluid. With a hooked needle the section is fished up from this fluid, rinsed in pure water, lightly poised over a bit of

blotting paper to absorb superfluous water, and laid flat in a drop of glycerine on the slide. It is then carefully covered with the thin glass, and is ready for examination. If it is wished to preserve the specimen, the glycerine which may have oozed out from under the covering glass must be carefully wiped off with a damp handkerchief or bit of wet blotting paper, and Canada balsam, gold size, Bell's cement, or any other ordinary cement lightly varnished round the edges of the cover.

"In thus advocating the working with sections, however, I am anxious not to underrate (as it is just now somewhat the fashion to do) the value of scrapings where more elaborate examination cannot be had. There is unquestionably much information to be gained from such stray elements as are to be scraped up from the freshly-cut surface; and when one has to give a diagnosis at once, a very shrewd guess can be made by comparing the naked-eye characters of the tumour and its mode of growth with the appearances presented by its juice. In fact, the moment a tumour is cut into, a scraping of its cut surface should always be examined with a $\frac{1}{4}$ -inch glass, for this will often make further examination unnecessary for purposes of diagnosis, and in doubtful cases may render incalculable service to the operator by informing him of the nature of the neoplasm, and of the expediency of removing perhaps more of the tissues around than might otherwise seem necessary."

Anyone who has perused the admirable work by Mr. De Morgan on the origin of cancer, lately noticed in the pages of this Journal, will admit that much patient research lies before us ere the elementary and fundamental question can be satisfactorily answered—Is cancer a blood-disease? In attempting to solve this all-important question, it is obvious that little way can be made without a satisfactory system of classification. It is in this direction, by enabling different observers to compare observations, and to speak with confidence of cases presenting similar histological characteristics, that Mr. Arnott's work is of real value.

Lettsomian Lectures on the Pathology and Treatment of Some Diseases of the Liver. By S. O. HABERSHON, M.D., Lond., F.R.C.P.; Physician to and Lecturer at Guy's Hospital, &c. London: J. and A. Churchill. 1872. Pp. 91.

THE Lettsomian Lectures were delivered this year by Dr. Habershon, of Guy's Hospital, and we are very glad that he has been induced to publish them in a collected form, and thus, we hope,

extend their usefulness. They contain, within a brief compass, a large amount of practical information, which cannot fail to be of considerable value to every practitioner. The subjects treated of are arranged under three heads, viz.:—The liver and its nerves; the liver and its vessels; and the bile and the bile-ducts—the diseases of each class being comprised in a separate chapter, an arrangement which, besides possessing the advantage of clearness, has also a therapeutical bearing. Each lecture commences with a concise anatomical description respectively, of the nerves, vessels, and bile-ducts. In the first, Dr. Habershon lays stress on the importance of recognizing the influence of altered nerve-function in diabetes; in the transient glycosuria not infrequent in atonic dyspepsia; as a determining cause (in the form of mental depression) to cancerous disease of the liver, “just as a blow may determine the growth of a cancer in the breast;” in neuralgia of the liver; in causing fatty degeneration of it; as evidenced in the ordinary “bilious attack;” and in acute yellow atrophy. Holding these views, the most rational treatment would in such cases be directed to the nervous system, and should “consist in the removal of nervous disturbance and exhaustion.”

Neuralgic pain in the liver may, it is stated, arise from at least three different causes:—

“First, it may be a sensation of fulness and throbbing, and of distress at the scrobiculus cordis, which is due to distension of the right side of the heart. This state is relieved by mercurials with squill and digitalis; by purgatives and by diuretics; and, in fact, by any of those means which lessen the vascular strain on the right side of the heart.

“Secondly, the pain may be situated across the epigastric region, and is due to the gastric catarrh; the food is imperfectly digested, and becomes coated with a thick envelope of mucus; and flatulent distension is the result. The pain thus produced is often most distressing to the patient; the distension of the stomach impedes the action of the diaphragm, and embarrasses to a greater degree the crippled heart. Hæmorrhagic erosion may also be induced, and coffee-ground vomit. This symptom—pain—is lessened by the remedies already indicated, and also by the use of mineral acids, by nux vomica, by carbolic acid, &c., the diet being meanwhile carefully regulated.

“A third kind of pain is evidently of a neuralgic character; it is not angina pectoris, but it is abdominal; and I have noticed its locality as situated deeply behind the first part of the duodenum—severe, almost like gall-stone, but without jaundice or its other symptoms; it is not

connected with the stomach, for it is not affected by food, but paroxysmal, and recurring sometimes with great regularity. The remedies we have mentioned may be used to their full extent; mercury even to the verge of salivation, digitalis till it can no longer be borne, purgatives may be used freely, and the anasarca removed by puncturing the legs; but still this severe neurosis continues; it appears to be due to exhausted nerve-function, and of those nerves of which we have already spoken. Narcotics and anodynes afford the only means we possess of palliating this distressing symptom."

The second lecture is devoted to some of the diseased states connected with the vascular supply of the liver, and the efficacy of mercurial and saline purgatives, and of aperient mineral waters in cases of passive enlargement and congestion spoken of. In such affections we have found the chloride of ammonium, in doses of from ten to fifteen grains, as recommended for hepatitis in India by Surgeon Stewart of the 21st Fusiliers, to be of great service. Dr. Habershon believes that the operation of tapping for the removal of the ascitic fluid in cirrhosis, if performed at an early stage of the disease, promotes recovery and saves much time, sparing the patient a long continuance of medical treatment.

"It is quite true," he remarks, "that at an early stage we are anxious to employ other measures less alarming to the patient; but in several instances in which it has been performed early, patients have soon recovered without the re-collection of the fluid, whereas in the later stages paracentesis is always attended with danger, for the peritoneal vessels are intensely congested, and peritonitis is easily induced. Many patients with chronic cirrhosis die a few days after tapping. Peritonitis does not in these cases produce pain, but rather typhoid exhaustion. The continued use of saline aperient waters is often attended with the greatest benefit in this form of chronic disease of the liver."

This question of early tapping appears to us an important point, and worthy of serious attention. As it is, paracentesis is rarely performed until respiration is impeded and nothing else remains to be done. Dr. Habershon gives the case of a woman who had been suffering from the disease for altogether about eight weeks, and who subsequently, after the removal of seventeen pints of fluid, made a rapid recovery. By affording a possibility of treating the disease at its commencement, and by saving time and the patient's strength, this method would seem to offer, in suitable cases, the best means of endeavouring to arrest the degenerative

process in its earlier stages. Dr. Habershon's experience of the danger of tapping in the later stages differs from that of Frerich's, who only saw inflammation of the peritoneum follow paracentesis in two cases, and therefore considers the operation rarely dangerous from its supervention.

In the last lecture different forms of occlusion and catarrh of the bile-ducts, and the subject of gall-stones, are instructively discussed—a class of “diseases in which alkalies are often of such signal service to the patient.”

The interesting subject of the association of disease is exemplified by observations on the occurrence of gall-stone with phthisical disease of the lung; with cancerous disease of the liver or gall-bladder—the irritation set up by the presence of the stone probably being the determining cause of cancerous deposition in the neighbourhood; and with pleuritic effusion on the right side, taking place a few days at least after the onset of the intense pain and jaundice.

We again strongly recommend this most useful little book to our readers: its practical value may be fairly gathered from our concluding extract:—

“The treatment of gall-stone should be divided into that which is calculated to relieve the paroxysm; that which lessens the jaundice; and thirdly, that which is designed to prevent the recurrence of the attack. As to the first, the intensity of the pain calls for immediate attention, and, by means of the hypodermic injection of morphia and the inhalation of chloroform, we are enabled to afford considerable relief; these means are much more effective and better than the internal use of opium, which is with difficulty absorbed, and has sometimes been given in such large doses as to endanger the life of the patient. Externally hot fomentations may be applied, or, what is more effectual, the mixed chloroform liniment, belladonna liniment, and aconite liniment—half an ounce of the two first, and a drachm of the latter. If the bowels are confined, they should be acted upon by a free mercurial purgative and warm saline draught, or by an enema.

“In hastening the removal of the jaundice, an unstimulating diet and gentle action on the bowels are the best means to employ; and the saline mineral waters are often of great assistance, but must be administered with caution. Alkalies may be used with advantage, not only in facilitating the discharge of inspissated bile, but in lessening duodenal irritation. It is of great importance also where other calculi are retained, and also where much irritation has been left after the passage of a calculus, that there should be no fresh source of irritation to the pyloric

region of the stomach and the first portion of the duodenum. Bismuth with alkalies is of some value in diminishing this gastric sensibility; but, whilst anæsthetics and anodynes afford immediate relief, and alkalies promote recovery, a great amount of patience is required by the medical attendant, as well as by the patient, lest the disease be aggravated by over-active treatment."

On some Affections of the Liver and Intestinal Canal, with Remarks on Ague and its Sequelæ, Scurvy, Purpura, &c. By STEPHEN H. WARD, M.D., Lond., F.R.C.P.; Physician to the Seamen's Hospital, to the Hospital for Diseases of the Chest, Victoria Park, &c. London: J. and A. Churchill. 1872. Pp. 260.

THIS book consists mainly of a collection of selected cases, illustrating different varieties and forms of some affections of the liver and intestinal canal, with chapters on ague, scurvy, &c., as observed, chiefly, in the Seamen's Hospital, interspersed with observations on diagnosis and treatment, and plentifully interlarded with the opinions of, and extracts from, various authorities.

The diseases of the liver treated of, comprise abscess, cirrhosis, hydatids and jaundice; and the remaining subjects include dysentery, peritonitis (with which is classed perityphilitis), ague, leucæmia, scurvy, and purpura. Emanating, as the volume presumably does, from what may be considered a special hospital, one might perhaps expect some new and valuable information derived from the study of particular diseases affecting a distinct class of the community. Anyone, however, who may take it up with such an idea will be disappointed, as, beyond the chapters on ague and scurvy, we have failed to find anything in Dr. Ward's book further than a faithful description of diseases that may be observed in almost any metropolitan hospital. Indeed, several of our author's cases—many of which are very instructive and ably reported—are derived from his private practice, and from the Hospital for Diseases of the Chest.

Dr. Ward's views on etiology, pathology and treatment, are, for the most part, identical with the teaching of the text-books of the day, notably Niemeyer's. We must, however, take exception to his treatment of jaundice, as in six out of the eight cases of that disease the histories of which are detailed, mercury was given until its specific effects were produced on the mouth, although subsequently,

and, we think, more correctly than his practice would seem to indicate, Dr. Ward states, that "in any case it is well not to give it too frequently, or (*sic*) to continue it for too long a time, for its protracted use is apt to impair digestion and nutrition, and even weaken the function of the organ which it at first relieves."

The experience of the Seamen's Hospital has led to the conclusion that special remedial agents are but of little use in the control or cure of dysentery, and that rest in a recumbent position, with a broad flannel roller around the abdomen, and a carefully selected milk and farinaceous diet, will, in many cases, alone effect a cure. An occasional dose of castor-oil and laudanum, or an injection of gruel with oil, and starch and opium enemata, remove the distressing symptoms caused by retention of scybala, and tenesmus. Hepatic abscess has not occurred in five per cent. of the cases of well-marked and severe dysentery.

Ague, and the resulting cachexia and sequelæ of it and of malarious remittent fever, constitute about one-tenth of all the cases admitted into the medical wards, and a fourth of these sufferers from malaria come from the low-lying districts of the Thames and other British ports. The largest number of recent cases occur in September: in such, quinine is not given until the tongue is fairly clean, and the stomach free from irritability. The minimum dose required has been fixed at ten grains, and three hours before the attack has been found the best time for its administration. The following sensible remarks with regard to the relative value of reputed anti-periodics which have been used as cheap substitutes for quinine, deserve the attention of our hospital authorities:—

"And here I must protest against that false economy which regards the relative value of drugs. Efficacy, not expense, should be regarded. It is always the best economy in an hospital to cure as quickly as possible, and there is the higher consideration of not allowing the patient's constitution to be under the damaging influence longer than we can help."

The chapter on scurvy gives an excellent description of a disease which, much to the disgrace of our mercantile marine, still exists in it. From an analysis of the relative frequency of the prominent symptoms, we find that in seventy-four out of one hundred cases the gums were affected, and that in fifty-eight there was more or less fibrinous effusion, chiefly about the knees. Mr. Harry Leach, to whose praiseworthy efforts, under the Board of Trade, in the field of mercantile naval hygiene, British seamen owe so

much, contributes to this chapter some interesting remarks on the disease. He thinks that the use of condensed water for drinking purposes may probably be a predisposing cause of scurvy a theory which is partly borne out by the large proportion of cases in ships whose crews have exclusively used rain or condensed water, and its frequency in vessels bound from Aden, where condensed water is supplied to most ships.

Dr. Ward's book is "of a purely practical character," and as such, will, by a perusal of the accurate descriptions of the diseases treated of, repay the reader.

The Dangers of Chloroform, and the Safety and Efficiency of Ether, as an Agent in securing the avoidance of pain in Surgical Operations. By J. MORGAN, M.D., F.R.C.S., Professor of Surgical and Descriptive Anatomy, Royal College of Surgeons, Ireland; Surgeon to Mercer's Hospital, Dublin, &c. London, Baillière, Tindall & Cox. 1872.

WE do not think it necessary to notice this volume at length, as Dr. Morgan placed his views on the important subject of which it treats, before our readers, in the November number of this Journal. There is no doubt that the inhalation of Chloroform, so as to produce insensibility, is not unattended with risk, and that, however much caution on the part of those who use it may diminish this risk, no means as yet suggested are capable of rendering its administration altogether devoid of danger; it has retained its place in surgical and obstetrical practice, because the benefits it conferred were so great as to make it worth the while of those who were compelled to submit to operations to run the risk; but if it can be shown that any other vapour will produce insensibility with anything like the convenience of Chloroform, and without the hazard which attends its use, the profession will gladly adopt it. Dr. Morgan has taken up the subject with the energy, enthusiasm, and ability which characterize all his efforts; the various papers he has written have attracted much attention, both in this country and England, and many of our most distinguished surgeons have entered on the discussion, and we hope, in a subsequent number, to present our readers with an impartial account of the facts and arguments brought forward.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

HENRY EAMES, M.D., Honorary Secretary.

Wednesday, November 20, 1872.

DR. HUDSON, President.

On the late Epidemic of Small-pox. By THOMAS DARBY, M.D., F.R.C.S.I.,
Surgeon to the Rathdown Workhouse Hospital.

THE valuable papers read to this Society during the height of the small-pox epidemic last session, and the interesting and important discussions in reference to that disease to which those papers gave rise, if they have not altogether exhausted the subject, have certainly rendered it difficult, after so short a lapse of time, to invest a communication on small-pox with much interest for the members of this Society. Nevertheless as those papers were chiefly based upon the disease as it appeared in the Metropolitan Hospitals, and as the hospital under my superintendence is situated in a rural district, I venture to ask your indulgence while I lay before you some statistics of the disease as it was presented to me, together with a few observations upon points which, so far as I know, have not hitherto attracted the attention they deserve.

On the 21st of February, 1871, the first, and on the 8th November, 1872, the last case included in this report, was sent into the Rathdown Hospital, and between those dates (both inclusive), 231 patients were treated in that institution. Of these 216 were sent in from various parts of the Union; 4 patients convalescent after fever; 2 servants in the hospital kitchen; and 9 inmates of the neighbouring workhouse took the disease. 168 had the specific mark of previous vaccination; 13 had been re-vaccinated; in 15 it was doubtful whether the individuals had ever

been vaccinated ; 34 had never been vaccinated at all ; and I had the pits of a former attack of natural small-pox.

Of the 169^a vaccinated, 15 died.

Of the 13 re-vaccinated 1 „

Of the 34 not vaccinated 13 „

Of the 15 doubtful cases 9 „

Total cases, 231 Total deaths, 38

To the usual classification of small-pox into the confluent, the semi-confluent, and the discrete, I would add the deep-seated, and the superficial, as constituting important distinctive varieties ; for although the true skin is the habitat of the true small-pox pustule, it cannot be doubted that whilst in some the pock is seated deeply in the substance of the corium, in others it is laid, so to speak, more lightly upon the villous surface ; indeed, judging from my own experience, I may say, that “ all other things being equal ” the depth of the pock may be taken as the measure of the fever, and that to this circumstance I attribute the amount of pitting which results to be due, rather than to the effect of any treatment adopted for the prevention of that deformity, for if there be but one deep pustule, do what we may, it will leave the inevitable pit ; whereas in the more superficial form of eruption, even though confluent, no pit may remain. Purpura, with and without hæmorrhage, occurred in a number of cases, and showed itself in three distinct forms—1st, in ecchymosed patches ; 2nd, in distinct blood spots in those parts of the skin unoccupied by pustules ; 3rd, where the pustules became filled with bloody fluid. Believing that purpura and scurvy are closely allied, if not identical affections, the treatment adopted in those cases consisted mainly of fresh lemon-juice and iron ; where hæmorrhage occurred, of turpentine or gallic acid. Several patients were affected with an uniform redness (erythema) over the entire surface, quite distinct from the pock ; of these some suffered from an erysipelatous condition of the fauces, in others again there was a peculiar pale form of pock, which, for want of a better term, I will call the lead-coloured eruption ; in one of those so affected, a patch the size of a man's hand on the pectoral region was studded by vesicles filled with a fluid as white as milk ; in another, where a sinapism had been applied to the epigastrium before the eruption appeared, the surface reddened by it, became covered with minute vesicles, which as the case progressed, coalesced, and formed one immense bag of lead-coloured fluid. I am unable to account for this peculiarity of colour, but it grieves me to say that every case affected by it died. Laryngitis was very formidable ; early leeching affording the only chance of saving life. In protracted cases diarrhœa proved very obstinate and

^a Including the case in which there had been a former attack of natural small-pox.

was best treated by sulphuric acid, given in infusion of bark aided by opiate enemata. In most cases where the fever ran high, delirium was a frequent concomitant, but taken *per se*, was not considered a very fatal complication. One case admitted from the Workhouse Infirmary in an advanced stage of lung disease, died of that affection rather than of the small-pox, which latter presented itself in a very mild form. The action of the heart was so feeble during the early stage of convalescence in the more severe cases, that in addition to the administration of tonics, such as strychnine, quinine, or iron, it became necessary to impose perfect rest, and to caution the patient against the danger of bodily exertion. In one instance where this was not sufficiently attended to, Dr. Mayne, who was in the infirmary at the time, was hurriedly called to the Small-pox Hospital to see a convalescent child, six years old, who, having been allowed to play with its doll, fell suddenly back in the bed and expired, I having seen it apparently doing well only a few minutes previously. Dr. Mayne, who, I hope, is now present, and to whom I am much indebted for the unremitting and efficient assistance he rendered me for many months, during the most trying and anxious period of the late epidemic, and who has considerable experience in the treatment of small-pox, can give you the particulars of one or two cases of great interest, which I saw in consultation with him, in which purpura and a weak heart were the prominent features. In a disease presenting so many and such varied complications, it is very difficult, if not impossible, to fix upon any one of them in particular as being the most frequent immediate cause of death, but of those who died in the earlier stages of the disease, laryngitis appears to me to stand pre-eminent; some died of hæmorrhage, and a few of the more protracted cases sunk from exhaustion; in no case was there any symptom of internal abscess either of the brain or other viscera, nor could I perceive any evidence whatever of the effete matter of the eruption having been absorbed back into the system. I am an unbeliever in the septic theory, and consequently did not resort to the use of what are called antiseptics in the management of those committed to my care; but as you may perceive, from what I have already stated, my treatment was guided by symptoms, my great object being to support the patient through a disease, the very nature of which is to run its course and cease at a given period. To effect this, in addition to the ordinary nourishment of the sick room, stimulants were freely administered, some patients having taken as much as eight ounces of whiskey, with from one to three pints of porter daily, for many consecutive days. The most marked benefit and comfort was secured to the sufferers by wrapping them in a sheet containing a quantity of flour when the maturated pock began to burst and discharge; additional flour being added from time to time so as to prevent the skin from sticking to the bedclothes. It would be

out of place here, were I to enter upon the question as to the causes which influence the spread of disease; but I cannot avoid while treating of the late epidemic, reiterating an opinion to which I have already given expression in this room, that however confident we may be that certain diseases are communicable from the sick to the healthy, we must also feel satisfied that some other influence besides contagion, be that what it may, does exist which is competent not only to cause the spread of, but also to originate those diseases. If it be a fact, as I believe it to be, that epidemic disease presents peculiarities in Ireland differing materially from what is observed in other parts of the United Kingdom, it is deeply to be regretted that the authorities in this country have not considered it expedient to acquiesce in the prayer of a memorial presented to them on a recent occasion by the council of the Irish Medical Association, backed up as that memorial was by the Colleges of Physicians and Surgeons, soliciting that an inquiry by Royal Commission into the state of public health in Ireland might be instituted before any Bill upon so important a subject should be submitted to Parliament, for it would appear as though public opinion is at present disposed to rest satisfied upon the hypothesis that contagion alone, mediate or immediate, is sufficient to account for the spread of epidemic disease—a narrow view of a very wide subject, at variance with the unbiased experience of every observant man, calculated to render any law passed under its influence costly to the community, and defective in operation.

DR. MAYNE said, as he had been referred to by Dr. Darby, he would briefly lay before the Society the prominent features of the cases which had come under his observation. The first was a case of the hæmorrhagic variety of purpura in combination with small-pox, which possessed all the features of hæmorrhage to a remarkable degree. He was called to see a female, forty years of age, the other members of whose family were suffering from small-pox. He was struck with the extreme symptoms of exhaustion which she presented. He perceived a large patch of purpura at the axilla. He ordered stimulants, and put her on iron. On the next day he was horrified at her appearance. She was sitting up in bed, supported by her sister, and there was a basin half full of blood which she was spitting up. Her eyelids contained a mass of fluid blood, and seemed as if they would burst; the conjunctiva were injected, and there was a large amount of hæmorrhage from the vagina. He treated her with stimulants, and gave her gallic acid, with the effect of almost immediately checking the hæmorrhage. She made a happy recovery, which was temporarily interrupted by two or three fits of syncope, from weak action of the heart; but she did very well. The next case was one of purpura, occurring in a female twenty-four years of age.

There was no hæmorrhage in that case, and the patient was exceedingly exhausted when he saw her. She had large patches, like thumb-marks, of purpura all over her body, and a general blue appearance, almost like cyanosis. This woman died the next day, without having had any hæmorrhage or any small-pox. As there were small-pox patients in the next room, he thought the cause of death was the poison of small-pox which produced its fatal result before the symptoms had had time to develop themselves.

DR. HENRY KENNEDY believed that of the different kinds of small-pox, the purpuric variety was the most formidable, and yet it had happened to himself that the cases he saw of it were, comparatively speaking, favourable, inasmuch as a large number of them recovered. He thought the uterine hæmorrhage was, comparatively speaking, not dangerous. It was a common form of hæmorrhage to meet, and yet a number of cases occurred to himself, as well as to others, in which the patients recovered. Hæmorrhage from the mucous surfaces, from the kidneys, and from the bowels, was very serious, but when it appeared on the surface of the body the prospect was more favourable. He had seen several cases where there were large purpuric spots on the surface of the body, and yet the persons did remarkably well. Very lately he saw such a case under the care of Dr. Moore, in the Cork-street Hospital. He anticipated that the individual would recover, and yet there were very large phlyctæna filled with blood, and many bloody spots over the body. While he had charge of Dun's Hospital a remarkable case occurred of an individual who went through a very severe attack of small-pox, of that kind of eruption where the pustules ran together, with a very livid, slaty appearance. There was a large, regular, uniform mass over the entire front of the stomach. That individual went through an attack of small-pox, getting a large amount of stimulants, and was going on well when she was attacked suddenly with severe hæmorrhage from the bowels, and it was feared she would die, but she recovered. At that time the eruption had entirely faded from the body, and she seemed to be doing well. The case was, therefore, a very remarkable one, for the period at which the hæmorrhage occurred. His own impression was in favour of the acid treatment—sulphuric acid, or sulphurous acid combined, and also with some opium. He had a great opinion of opium in those cases; he had taken the hint from Sydenham, and had never had reason to regret acting upon it. He had seen a case with extreme hæmorrhage from throat, nose, and uterus, and from the time the patient began to take opium she got better. He had seen another case with Dr. Stokes, where there was hæmorrhage from the eyes, uterus, and bowels, but all treatment there was useless. The worst cases of purpura with small-pox occurred in women, many of

them of very robust appearance, fat, and of high colour. There was another strange feature that struck him through the whole of this epidemic, and that was the little sympathy that the pulse showed with the severity of the symptoms. In scarlatina of a serious type, and in other fevers, there was always a bad pulse, but in small-pox they would have a pulse of 100, reasonably strong, and yet the patient was certainly going on to death. This struck him as very remarkable, and had it not been for the experience gained in other cases, one might be entirely thrown off their guard as regards the prognosis.

DR. GRIMSHAW wished to know, with regard to the thirteen re-vaccinated cases spoken of by Dr. Darby—which seemed to be a large number—whether the patients had the marks of two vaccinations?

DR. DARBY.—They had.

DR. GRIMSHAW said that, at the time he read a paper on the subject, there had been 400 cases of small-pox in the Cork-street Hospital, without a single case of re-vaccination, and since then there had been as many cases—about 900 in all—and of these there were only three cases of re-vaccination. One was the case of a girl who was taken in to nurse her little brother, and she then got small-pox, and there were only nine pustules on her body. The two other cases were brothers; one had been re-vaccinated several times, and he died of purpuric small-pox; the other recovered; but this would point to some peculiarity in the case. Therefore the 13 cases of Dr. Darby were remarkable, and he would like to know whether there were any peculiar circumstances to account for so large a number.

DR. DARBY said that all the cases to which he had referred presented the double marks of vaccination. They were re-vaccinated at different periods. They were all, with the exception of one case that died, of a mild form. He had never seen a specific mark after re-vaccination, when the primary mark was a good one, and he had re-vaccinated many hundreds for the last thirty years, re-vaccinating many in private practice as well as those in the workhouse. He knew a lady who had been vaccinated seven times, and had had small-pox seven times. She was a lady of rank, and he was re-vaccinating some members of her family when she told him the story. She was vaccinated first, as an infant, by the late Dr. Labatt; she was afterwards vaccinated by Dr. Pugh, of London; afterwards by Dr. Dwyer, of the Dublin Cow Pock Institution, and several times subsequently. When he was vaccinating some of the members of the family, during the late epidemic, she said—“This is great nonsense you are going on with, because I have been

vaccinated seven times, and every time there has been small-pox present I take it, and I have it now;" and she showed him her arm with the small-pox pustule upon it, she being in the drawing-room with her friends at the time. Where the natural pock will not protect an individual, it was not natural to suppose that vaccination would do so in a similar case. There could be no doubt, however, as to the efficacy of vaccination. The proportion of those who were vaccinated, who took small-pox, was 1 in 11, whereas of those who were not vaccinated, it was 1 in 2.

DR. HEAD asked were the cases spoken of by Dr. Darby recent re-vaccinations?

DR. DARBY said some were within a month, some about a year, and some five, six, or eight years previously. A child of two months old was brought in in her mother's arms, the mother being in small-pox at the time some six or seven days. He vaccinated it immediately, but it took the disease and went through it in a modified form.

The Society then adjourned to the second Wednesday in next month.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIFTH ANNUAL SESSION.

JOHN R. KIRKPATRICK, M.B., Honorary Secretary.

Opening Meeting, Saturday, 23rd November, 1872.

The President, DR. KIDD, in the Chair.

THE Report of the Council having been read, and the Officers for the ensuing Session elected, Dr. KIDD, the out-going President, read the following address before leaving the Chair:—

GENTLEMEN,—In the report presented to the Society at the commencement of the past session the council recommended that the President should for the future be eligible for re-election at the termination of his first year of office, and it pleased you to adopt the recommendation, and to confer on me for the second time the honour of being your President. Therefore it is that I have now to again appear before you with an opening address.

We enter to-night on the thirty-fifth annual session of our Society, and it affords me great pleasure to be able to congratulate my fellow-members on the soundness of our position and the vigour of our proceedings. The report that has just been read shows that, as regards our finances, we are prosperous, and the volume of our proceedings which is now about to be distributed to the members will be found, I do not hesitate to say, to contain papers which will take a high rank in the literature of our branch of the art and science of medicine. When we met last year your council announced that the time had arrived when the publication of this volume might be safely undertaken. Owing in a great degree to the facilities afforded to us by the publishers of the Dublin Journal of Medical Science your council have been enabled to bring this undertaking to a successful issue, and thus to begin what

they hope will be a long continued series of reports. The labour imposed on the council, but more particularly on your secretary, Dr. Atthill, in bringing out the volume, has been very great, for it required and received constant care and attention, but now that the model has been formed, the publication of future volumes will not be attended with so much difficulty, and I trust the members of the Society will unite in the determination to make our proceedings such as will not only be worthy of the reputation of the Dublin school, but such as will increase and extend its fame.

It would be a great happiness to all of us, gentlemen, if in referring to the past session of our Society we could stop after recounting our triumphs, and had no occasion to refer to losses sustained, and to empty places in our ranks that we can never hope to see filled again. It is true that the deaths that have occurred among us have not been many, for we have, I believe, lost but two of our number during the year, but in Thomas Edward Beatty we have lost one who was in himself a host—one whom we all loved and all delighted to honour. He was one of the earliest members of this Society, assisted it in its early struggles, supported it when it was in adversity, and rejoiced over it when it attained its present prosperity; a prosperity to the accomplishment of which he himself contributed in no small degree. In 1855, seven years before the management of the Society was thrown open, he was elected one of the presidents, an office which was then held for life, and when the amended constitution was adopted in 1862, the members exercised their newly acquired powers by electing him as their first annual President. To the last day of his life the interest he took in the Society never lessened; never, when he could avoid it, was he absent from our meetings, he rejoiced to assist at them, and to take part in our debates, and was always ready to give us the benefit of his vast experience. The esteem in which he was held was evidenced by the honours conferred on him, not alone by our Society but by the whole profession. He was elected one after the other to all the highest positions in its ranks. I have already said he was one whom we all delighted to honour. Of this he was deeply conscious. It was to him a great gratification—and of it he was justly proud. In the preface to the volume published in 1866 he alluded to it in the following terms:—"It cannot," he said, speaking of publishing the book, "be laid to my charge that I have adopted this method of bringing myself before my professional brethren with a view to future reward or promotion, for their unbounded favours have left me nothing more to wish for, or to which I could aspire. The many evidences of the esteem of my brethren in both branches of our profession with which I have been honoured might well satisfy the most soaring ambition, and afford grounds for the deepest thankfulness, which I now desire to

express, and again I repeat that my cup of professional honours is full, even to overflowing."

On the 19th of April last Dr. Beatty took a prominent part in a debate at the Surgical Society on diffuse inflammation of the neck, and a few days afterwards became himself the subject of this disease. The inflammation ran a rapid course, and in less than a week, on the 3rd of May, he whom we loved so much came to be counted with those whose places knew them no more. "Silver-toned, gentle-handed, warm-hearted, clear-headed, genial, learned, scientific, staunch, hospitable 'Tom Beatty' will no longer rule in council, cheer the sick bed, instruct with his great practical experience, nor delight the social circle." It was in these words from the pen, I believe, of Sir Wm. Wilde, that one of our morning papers communicated to us the news of his death, and I have no doubt you will all recognize the accuracy of the picture.

But one other member of our Society died during the year. This was Mr. Dirham. Though a very regular attendant, he never took an active part at our meetings. He was for several years a member of the Court of Examiners, and at the time of his death the Deputy Governor of the Apothecaries' Hall.

In the list of our honorary members one name more appears with the mark to indicate that its owner has departed from amongst us. The name of one who, like Beatty, always stood high in our esteem and admiration. Friends and fellow workers, Beatty and Simpson, they may be regarded as representative men. To be familiar with their works is to be familiar with all that is best worth knowing in obstetric and gynaecological medicine. To recount what they have done would be to describe the greater part of the progress that the last thirty years have witnessed in our branch of the healing art. This would be a task far beyond the limits of the present address; but there are certain salient points at which it may not be unprofitable to glance. Without attempting any comparison, the two men, Beatty and Simpson, may be classed together. They were almost contemporary. Beatty, the older man, was born in January, 1800, and died on the 3rd May, 1872. Simpson was born in 1811, and died in May, 1870. They both possessed great intellectual power, and great mechanical ingenuity. They had unbounded zeal and unfailing industry, and, though devoting themselves mainly to obstetric medicine, they both contributed to our knowledge of other branches of medicine, and gave us new stand-points from which further progress has been attained.

Simpson was a voluminous writer, Beatty not nearly so much so. Neither of them ever published a systematic treatise. Their writings, for the most part, took the form of detached essays, published from time to time in the medical journals; but which have since been collected and

republished. The subjects on which they wrote were often parallel. Beatty himself, thought his papers on the Forceps, Ergot of Rye, Chloroform, Cancer of the Uterus, and Abdominal Aneurism, the most important—with these it was his ambition that his name should be associated. Simpson's name is, perhaps, best known in connexion with his papers on Chloroform. These attracted much popular attention, but several others are, in a professional point of view, more valuable, especially I would say those on the Use of the Uterine Sound, and on the Diagnosis of the Diseases of Women.

In the remarks I had the honour last year to address to you from this chair, I dwelt at sufficient length on the part taken by Beatty, following in the footsteps of his father and assisted by Dr. Churchill, in re-introducing the use of the forceps into Irish midwifery. It is not necessary to recur to this now, so I shall pass on to the papers on chloroform.

The production of anæsthesia or insensibility to pain by the inhalation of chloroform, was, unquestionably, the discovery of Simpson. By some very inaccurate thinkers he has been accused of having claimed for himself to have discovered the power to produce anæsthesia. Even a superficial examination of his writings would refute this; but the charge was brought against him in such a manner that from his death bed he replied to it in definite terms. He showed that the means of producing insensibility to pain had long been sought for. That from so early a period as 700 years before Christ we have records of this. That at the end of the last century Davy showed that insensibility might be induced by the inhalation of nitrous oxide, and some thirty years afterwards Faraday, in England, and Godman, in America, found that the inhalation of the vapour of sulphuric ether produced a similar effect. That on the 11th of December, 1844, Dr. Wells, an American dentist, had two teeth drawn from himself while he was insensible from the inhalation of nitrous oxide, and on the 20th of September, 1846, Morton, at Boston, drew a tooth from Eben Frost while he was insensible from the use of sulphuric ether.

Thus Simpson has shown that, like many other discoveries, our knowledge of the means of producing anæsthesia has been a gradual growth, but up to this point it was thought that insensibility could only be produced for operations brief in their duration, whatever might be their severity. Its application to midwifery involved many more difficult and delicate problems than its mere application to dentistry and surgery. To these problems Simpson applied himself, and on the 19th of January, 1847, for the first time anæsthesia was produced in midwifery, the agent used being sulphuric ether, and the patient under his care. He found, however, great disadvantages attendant on the use of ether, and entered on a series of experiments in search of a more manageable and con-

venient agent, and on the 15th November, 1847, discovered the anæsthetic powers of chloroform.

The extent to which the agent thus discovered is made use of is proved by the fact mentioned by Sir James Simpson that one house in Edinburgh manufacture between two and three million doses of chloroform every year.

Immediately on the publication of the statement that patients might be rendered insensible to the pains of labour, Beatty applied himself to investigate the safety of the practice, and he was one of the earliest to adopt the use of chloroform. It was found, however, that uterine action was lessened, and that there was an increased liability to post-partum hæmorrhage from the use of chloroform. We owe to Beatty the suggestion that this may be obviated by preceding its administration by a full dose of ergot of rye, and in this way Beatty supplemented and made more valuable the discovery of Simpson.

It has been stated lately that "chloroform is the most dangerous of all anæsthetics;" whether this will ultimately prove to be true I cannot undertake to say, but it is worthy of remark that no death has been recorded as having occurred from its use during labour; and it seems very probable that the danger arising from its use for other purposes has been greatly exaggerated. Certainly, if we can judge of it by our experience in Dublin, this must be so. It has been stated that "one death from its use occurs in these countries (meaning, I presume, the United Kingdom) every week;" but in Dublin, so far as I can learn, only five deaths* have been attributed to it during the twenty-five years it has been in use, both in public and in private practice; and of these one at least was probably due to the admission of air into the veins and not to the chloroform.

Simpson's Memoir on the Use of the Uterine Sound has always appeared to me the most valuable contribution to gynæcology of the age in which we live. It may be true that an instrument analogous to the sound has been found among the ruins of Herculaneum or Pompei, and is even alluded to in the writings of Hippocrates, and that Lair Valleix and others used bougies and other instruments for measuring the uterus, but it is to Simpson we owe the memoir that taught us all how to use the sound, and laid the foundation of almost all our subsequently gained knowledge of uterine disease. What has most contributed to the

* Since this was written, I have heard of a sixth death. Three of these occurred in hospital, and three in private practice. The cases in hospital occurred during an operation for cataract, an attempt to reduce a dislocation of the shoulder joint, and the removal of a tumour. The cases in private occurred during an attempt to induce premature labour by Kiwisch's douche, the extraction of a tooth, and an operation on the jaw.

progress of modern medicine has unquestionably been the attention paid to physical signs and the improvements effected in the instruments at our disposal for recognizing and distinguishing the varying conditions of internal organs. The advances made in our knowledge of the pathology and treatment of diseases of the organs in the thorax since the publication of Lænnec's great treatise *On Auscultation*, have created a revolution in this department of medicine. Simpson's *Memoir on the Uterine Sound* holds, in my humble opinion, the same relation to uterine disease that Lænnec's *Treatise on Auscultation* holds to thoracic disease. It also has created a revolution in the department of medicine to which it belongs.

Till a comparatively recent period our knowledge of the diseases of the uterus and its appendages was in a much more deplorable state than was our knowledge of thoracic disease before Lænnec's treatise appeared. The speculum vaginæ which had been well known to the ancients, and was accurately described some 2,000 years ago, had fallen into absolute disuse till revived by Recamier early in the present century, but even by its aid we can learn but little beyond the appearance of the vagina and neck of the uterus. The use of the sound carries us very much further. It increases to a great degree our power of making a perfect and precise tactile examination of the fundus, body, and cervix of the uterus. It facilitates and simplifies the visual examination of the cervix of the uterus with the speculum. It enables us in many instances to ascertain the connexion or non-connexion of hypogastric tumours with the uterus. It affords us valuable diagnostic information by enabling us to measure the length of the uterine cavity, and by its use we can learn the position of the uterus and its relation to surrounding parts. We have thus then, I assert, in the sound, an instrument as valuable in reference to uterine disease as is the stethoscope to thoracic disease.

Many other contributions of Simpson's are only inferior in value to the *Memoir on the Sound*. To him we are indebted for nearly all we know of flexions and displacements of the uterus, and that peculiar condition so productive of discomfort and bad health, which he named sub-involution of the uterus, was not known till he recognized and described it. Then again it is to him we are indebted in the main for one of our greatest improvements in the treatment of uterine disease. I mean the getting at the interior of the uterus so as to apply treatment directly to the diseased surface. The use of prepared sponge and other expanding tents for dilating the uterus is described by the earliest writers; but till Simpson directed attention to it, complete exploration of the cavity of the uterus was not known in modern practice. We can now explore the interior of the uterus, ascertain its condition, make applications to its surface, and even perform surgical operations within it. We have thus, by the genius of

this great man, had the power conferred upon us of coping with a large class of diseases formerly almost absolutely beyond our reach, such as chronic catarrhal discharges, hemorrhages, intra-uterine polypi, fibrous tumours, and others.

Beatty wrote many papers on physiological and medico-legal questions. Of his paper on Aneurism of the Abdominal Aorta Dr. Stokes has said :—“Our knowledge of the diagnosis of this disease may be safely said to date from the year 1830, when Dr. Beatty, of this city, published his accurate observations on a single case of the disease.” His paper on frottement in peritonitis is also one of great value; and that on cancer of the uterus has added greatly to our knowledge of this disease. Simpson, too, published many papers on physiology and questions of medico-legal importance. His treatise on acupressure is one the full value of which is not yet known, but already it has opened up questions of great gravity, and modified in a marked manner the practice of surgeons in the treatment of wounds. It has been well said of him that “in scholarship, in antiquarian lore, and in extent of private practice, he had few equals in our profession, and rarely have men earned such distinction as he did out of their ordinary walk in life.”

In the month of October, 1869, the citizens of Edinburgh assembled and presented Sir James Simpson with the freedom of the city; and on the 13th of May following, they accompanied his remains to the grave with all the pomp and ceremonial of a public funeral; and yet, strange to say, we have had, from the same Edinburgh, a series of attacks* on his fame and reputation, so gross in their character, that if it were not for the hitherto high standing of the journal in which they have appeared, I should not condescend to notice them here; but when a journal of such standing as the Edinburgh Medical Journal sanctions, with its authority, the publication of such unjust articles, it is not only right but necessary that all honest men should protest against them. It is especially necessary that we, as members of this Society, should do so; for if we elected the subject of them to the high position of one of our honorary members, and maintained him in that position, our honour is bound up with his. If it be true that the men we have chosen for honour are “not the noble men of the profession of medicine,” but “men of talents and ambition, who have good fortune, a quackish disposition, and the arts of a lady’s maid, who can always simper and dally with truth”—“men who have no time or stomach for laboriously gained experience, but have the brass of audacious pretension”—then are we ourselves disgraced. This anonymous and “audacious” writer, who has for the time gained access to the pages of a journal hitherto of the highest

* Edinburgh Medical Journal for Aug., 1871, p. 151; Oct. 1871, p. 547; Jan. 1872, p. 634; March, 1872, p. 831.

standing in medical literature, has not only applied such language to Sir James Simpson, but, in his haste to vilify and disparage the dead, has launched his diatribes against the whole profession, and even by name included in his denunciations Sir Charles Locock, whom we are still proud to have on our list of honorary members. Simpson, we are told by this writer, was greedy of money and greedy of reputation. He had, it is admitted, reputation; and it is common, we are told, to suppose that reputation of this kind cannot be produced without a good substantial basis; but this natural supposition is, according to this writer, a gross mistake, for the widest, and in some senses the largest, reputation may, he says, be based on nothing good or substantial.

The articles to which I allude abound in covert inuendos still more offensive than those more direct statements. The memoir on the uterine sound, of the value of which I have already spoken, is damned with faint applause. It is stated that "Simpson's claim to the discovery of chloroform has been contested," meaning, of course, the anæsthetic uses of chloroform; and it is hinted "that further evidence may yet be produced regarding it, as many of Simpson's contemporaries and collaborators still live who have published no documents or statements." This is a style of writing, especially when applied to one no longer with us to defend himself, that is both unfair and ungenerous; but when we find the same author insinuating in such words as the following, a comparison between Simpson and the medicine man of Africa, we cease to attach any importance to his words:—"The medicine man of Central Africa is," this writer says, "a conscientious creature, just as he is in Scotland. He has no doubt about his diversified therapeutic resources. Patients in Central Africa pay their fees and swallow their drugs, just as in Scotland. Such is polypharmacy as it still exists in Great Britain, and is still expounded in the end of the nineteenth century by great teachers."

I shall not detain you with further observations on these disreputable and disgraceful reviews. I felt it incumbent on me, not only for the sake of the profession at large, and for the honour of our Society, but because of many kindnesses received at his hands, to protest for myself, personally, and with all the weight the position I have occupied as President of this Society adds to my words, against this treatment of one whom we must all regard as a great man, a great physician, and a great leader in obstetric progress.

Saturday, 14th December, 1872.

The President, DR. EVORY KENNEDY, in the Chair.

On Endo-Metritis. By LOMBE ATTHILL, Fellow and Examiner in Midwifery, King and Queen's College of Physicians; Obstetric Physician to the Adelaide Hospital, and Vice-President of the Society.

ALL obstetric practitioners are familiar with the various forms of chronic disease of the cervix uteri, which are so commonly met with in practice, and which for a long period were the only forms of non-malignant uterine disease recognized or treated. Of late years our knowledge of the inflammatory affections to which the uterus is liable has been greatly extended, and we now recognize and successfully treat, not only chronic disease of the vaginal portion of the cervix and of the cervical canal (endo-cervicitis), but also inflammation of the lining membrane of the cavity of the uterus (endo-metritis), affections sometimes occurring independently, but frequently co-existing in the same subject.

Endo-metritis, formerly looked on as an affection of rare occurrence, is now, that its symptoms are better known, recognized as a disease of comparative frequency. It is met with in women who have never been pregnant; nay, more, I have seen a well-marked example of it in a virgin; but it occurs most frequently as a result of imperfect involution of the uterus, and in aggravated cases may terminate in complete disorganization of the intra-uterine mucous membrane. Such extreme cases are, however, rare. Sir James Simpson was the first to point out that the process of reduction in bulk and size which the uterus undergoes within a short period subsequent to delivery is frequently arrested, and that the organ consequently remains in a condition of chronic engorgement and enlargement. Obviously such a condition is most favourable to the occurrence of inflammation, which in point of fact does frequently supervene.

All cases of endo-metritis necessarily fall under two heads, namely, those in which the cervix is engaged, and those in which that portion of the organ is not implicated, or is so in a secondary degree. The former are, I think, the most numerous. They are also those which frequently mislead the unwary practitioner. He meets with a case in which there is an enlarged and thickened cervix, with a patulous os uteri, from which exudes a copious glairy discharge, indicative of endo-cervical mischief. He confines his treatment to the cervix, and perhaps cures the cervical catarrh, but is disappointed at finding that his patient's sufferings are but little diminished; the pain in the back, the pain felt along the margin of the false ribs, and perhaps the dysmenorrhœa are as acute as ever; his

treatment must extend further, or it is useless. On the other hand, if the cervix be healthy, the existence of endo-metritis may be overlooked. I have known numerous instances where patients were assured that no uterine disease existed, because the cervix when exposed by the speculum appeared healthy. I shall not in the present paper enter into the consideration of the subject of endo-cervicitis, for where this affection exists in conjunction with a diseased condition of the uterine cavity, the treatment proper for it will generally prove sufficient for the cure of the other more evident affection, while if it be overlooked, or recognized only on the subsidence of the endo-cervical disease, the treatment suited to the uncomplicated form must be resorted to; nor do I intend to enter into a discussion as to whether in such cases the endo-metritis be an extension upwards of an inflammation first attacking the cervix, for even were it possible to give a decided answer to this question it would have no practical influence on the treatment of the case.

It is, in the first place, necessary to define the sense in which I use the term endo-metritis. I understand by it inflammation of a low type of the mucous membrane lining the cavity of the uterus, with engorgement of its blood vessels, the glandular structure which it, in common with the lining membrane of the cervix, possesses, being implicated.

Endo-metritis presents two well-marked stages. In the first the body of the uterus is enlarged, the mucous membrane lining its cavity being congested and swollen, while the uterine walls are thickened, and the whole organ consequently becomes heavy. In the second stage the walls are thinned, the muscular structure is relaxed, and the cavity frequently enlarged, while the mucous lining becomes soft, spongy, and granular. In the first stage the intra-uterine discharge is pale in colour, inodorous, and fluid. In the second it is often rust-coloured, sanguinous, and sometimes even purulent. This latter is specially likely to be met with when the disease occurs in women of advanced age, in whom a copious purulent discharge is seen to issue from the os uteri. Of this I saw a well-marked example recently in an old lady aged seventy. She suffered much from general pelvic distress, with leucorrhœa. I found that the uterus, which was enlarged, was completely retroflected; the vagina was healthy; the leucorrhœa, which was copious and purulent in character, issued from the os uteri. Much relief was derived in this case from supporting the uterus with a Hodge's pessary. I deemed no further treatment judicious under existing circumstances.

The symptoms of endo-metritis, necessarily somewhat vague, are:—(1.) Pain; (2.) Leucorrhœa; (3.) Dysmenorrhœa, or, sometimes, Irregular Menstruation; (4.) Menorrhagia; (5.) Reflex irritation.

(1.) Uterine Leucorrhœa is invariably present, but it may escape observation, especially in the early stages, or it may be confounded with a vaginal

discharge. Even if a speculum be used no uterine discharge may be observed during the time that the cervix is exposed to view. Or again, if endo-cervicitis be present the copious glairy discharge which it gives origin to may prevent our recognizing the other, but in many cases it is copious and easily distinguished if only moderate care be exercised. In the advanced stage of the disease, when it becomes rust-coloured or purulent, no difficulty exists in detecting it, and I may here remark that the reddish discharge which sometimes accompanies this affection has in the old been mistaken for a return of menstruation. In the case of the old lady just referred to this was so. Rust-coloured, purulent, or offensive discharges issuing from the interior of the uterus may be taken as indicating extensive disorganization of the lining membrane of the body.

(2.) Pain is also invariably present. This in general is referred to one or all of three localities, namely, to the sacrum; to the edge of the false ribs, generally those of the left side, and shooting up to the shoulder, and to a point immediately over the pubes. This last is the least common. That referred to the edge of the false ribs I look on as being the most frequent, and also as being often almost pathognomic of the affection. I have remarked too that in cases of endo-metritis it becomes aggravated at the recurrence of each menstrual period.

(3.) Dysmenorrhœa, often of a severe character, is a nearly constant concomitant of inflammation of the intra-uterine mucous membrane, as this is frequently observed in women who have borne children and in whom the uterine sound passes with ease into the uterus; the supposition that this symptom is due to any mechanical obstruction as that which would be caused by swelling of the mucous membrane at the os internum is in such patients negatived. In them I believe it is due to increased sensibility of the uterus at the point of junction of the cervix and body. This view is confirmed by the fact that in such patients pain identical in character with that experienced at each menstrual period, is brought on when the point of the instrument reaches the spot indicated. But on the other hand, that form of mechanical dysmenorrhœa which is produced by swelling of the mucous membrane of the cervix uteri occurs in those multiparous women in whom a conical cervix and contracted cervical canal congenitally exists. In such patients menstruation frequently becomes painful after marriage, sexual intercourse evidently having in them produced congestion, which terminated in an inflamed and swollen condition of the lining membrane of the organ. In some cases too, the menstrual flow becomes irregular, and occasionally interrupted, appearing for a day then ceasing to reappear again.

(4.) Menorrhagia, in the advanced stage is often its most prominent feature, and not unfrequently are we consulted for it alone. Even if

absent in the early stages of the disease, it is almost certain to occur further on, when the mucous membrane having been for a long time engorged and inflamed, becomes covered with numerous vascular elevations, from which the bleeding proceeds, and which in many cases assumes an alarming character. An exact counterpart of these vascular elevations can sometimes be seen on the vaginal aspect of the cervix, and also in the rectum, in granular disease of the mucous membrane lining that intestine. Medicines administered by the mouth are therefore here as absolutely useless, as they are known to be in cases of hæmorrhage from the rectum depending on a similar cause, and if the disease be not treated, by means of applications made directly to the diseased surface, the hæmorrhage may continue for an indefinite time. I shall presently refer to a case in which uterine hæmorrhage depending on this cause continued for ten years.

(5.) Reflex irritations are generally present more or less markedly in all cases of endo-metritis, the most prominent being those of the bladder and stomach, the one producing frequent desire to micturate, the other giving rise to nausea and even vomiting. Occasionally too ovarian and mammary sympathies are excited: chiefly to be noted lest their presence should mislead and induce us to refer the patient's sufferings to a wrong cause.

The physical signs indicating the existence of endo-metritis are:—

(1) Increased length, or (2) increased size of the cavity of the uterus, and consequently (3) increased bulk of the whole fundus, (4) increased sensibility of the mucous membrane lining the cavity of the uterus, (5) a patulous os internum, and (6) often an abnormally sensitive condition of the mucous membrane at that point; lastly, displacements anteriorly, or posteriorly, of the fundus, resulting from the increased size and weight of the uterus are frequently observed.

(1) Increased length of the body can only be recognized by use of the sound; that at once tells us what the length of the entire organ may be; but it is necessary to discriminate between the elongation depending on enlargement of the body and that produced by cervical allongment; but as an ordinary digital examination always informs us what the length of the cervix is, moderate care will enable us to estimate correctly how much, if any, of the increased length is due to the body.

(2) It is much more difficult to determine the size of the cavity. If the sound can be rotated freely it may be surmised that it is enlarged, but what its actual size may be remains a matter of uncertainty.

Dr. Greenhagh has invented an ingenious instrument for the purpose of measuring the capacity of the uterine cavity. It can be expanded when introduced, and the extent of its expansion is registered by a very

simple apparatus. It is an instrument capable in some cases of affording useful information.

(3) The fundus has been considerably enlarged in all cases of endometritis that have come under my observation, a condition in general easily detected by the bi-manual method of examination.

(4) Abnormal sensibility of the mucous membrane lining the cavity of the uterus is a necessary accompaniment of disease of that membrane. That this is so can in general be proved by pressing the point of the sound when in the cavity against the fundus. This in a healthy uterus causes no pain, but where endo-metritis exist it is immediately complained of. It is a test to be used, however, with care, for in old standing cases the uterine walls become sometimes so thin that very little force is needed to make the point of the sound penetrate them; and I believe that this absence of resistance has misled some recent writers into supposing that the entire fundus of the uterus has occasionally been absorbed.

(5) In endo-metritis the os uteri internum is always patulous, the sound passes through it without difficulty; but (6) sometimes, as I have already pointed out, this, nevertheless, causes severe pain. In those cases where endo-cervicitis exists, the point of the sound may be entangled in a fold of the diseased mucous membrane, and its further progress arrested, but the difficulty thus produced is easily distinguished from the resistance due to a contracted or rigid os uteri internum.

The treatment of this troublesome and often most untractable affection necessarily is influenced by its duration, by the severity of its symptoms, and by the prominence of some special one.

If pain, nausea and general malaise be those which the patient mainly complains of, rest, warm hip baths, mild aperients, and above all the local abstraction of blood, will do much good, and sometimes even effect a cure. But if profuse or purulent leucorrhœa, or menorrhagia, be present, such treatment is at best merely palliative, and treatment applied directly to the diseased surface becomes imperatively called for, but in all cases where much tenderness on pressure exists, local blood-letting should be first practiced.

This is a rule from which I make few exceptions. Local blood-letting relieves, to a considerable degree, the pain, and certainly favours the action of other treatment, whether that be medicines administered by the mouth, or applications made directly to the diseased surface.

Local depletion is a very old practice of recognized value. It has, however, fallen into disuse, apparently because, when carried out by means of leeches, it is troublesome, and, moreover, is often attended with unpleasant consequences. Sometimes the leeches will not bite, at other times they will fasten on the vagina and give rise to bleeding alarming in quantity and difficult to stop. Sometimes, too, notwithstanding every

precaution, a leech will make its way into the os uteri. When this has occurred to myself, as it has on two occasions, the leech returned soon, but a patient assured me that on one occasion a leech remained in utero for twelve hours, and gave rise to no small anxiety. I admit, then, that for these reasons, the application of leeches to the cervix is objectionable, but they only prove that this mode of abstracting blood from the uterus is unsatisfactory. I am decidedly of opinion that, as a preliminary treatment, local depletion is most valuable; doubtless, it does not produce as well marked results in the treatment of endo-metritis as it does when practiced in cases of cervical congestion, and if relied on alone will cause disappointment; but I repeat again, as a preliminary measure, it should seldom be omitted.

I invariably practice local depletion by puncturing the cervix, using this knife. If the cervix be soft and spongy it must be used cautiously,



one or two punctures, one-eighth of an inch in depth will, generally, be followed by sufficiently free bleeding, if not, one or two more deeper ones should be made, and if the cervix be indurated, the point of the knife must be made to penetrate still deeper. The quantity of blood taken can thus be regulated with nicety, but a few minutes are occupied in the operation, and no pain is caused. The bleeding generally ceases the moment the speculum is withdrawn, if it should not, a pledget of cotton must be placed in the vagina, and left in situ for a few hours; but it is very rare, indeed, that even this is necessary. I again repeat that local depletion does not produce as beneficial results in cases of corporal endo-metritis as it does in cases of cervical congestion; the benefit, therefore, resulting from the practice will be in an exact ratio to the amount of cervical disease which may exist.

As already stated, local depletion is, in cases of endo-metritis, but a preliminary step; to effect a cure treatment must be adopted, which will act directly on the diseased surface—that is, on the mucous membrane lining the body of the uterus.

There are three methods of making applications to the interior of the uterus; one is the injecting of fluids into its cavity; another the introduction of a piece of solid caustic into the uterus, by means of Simpson's intra uterine, *porte caustique*; the third is the passing up to the fundus, a stilette armed with a layer of cotton or strip of lint saturated with nitric acid, with the acid nitrate of mercury, or some other active agent.

The first of these methods I have never tried, as it is a practice not free from danger, and not only so, but also much less certain and satisfactory in its results than either of the others.

The second I have frequently practised, in cases of imperfect involution of the uterus; where no inflammation exists its effects are most excellent, but it does not, so far as my experience goes, produce, by any means, so satisfactory results in the treatment of endo-metritis, as does nitric acid if properly applied.

The application of strong caustics to the interior of the uterus, of which, in my opinion, the fuming nitric acid is by far the best, is a practice now extensively carried out, not only in this city, but also in America. Our brethren, however, in England and Scotland seem as yet unacquainted with the advantages which follow this treatment. I am quite ignorant as to whom we are indebted for introducing into practice this method of treating chronic endo-metritis. Dr. Miller, of Louisville, U.S.A., in an excellent article published in the number of *The American Journal of Obstetrics* for August, 1871, claims to have been the first to advocate the application of remedies of this class to the interior of the uterus; his lectures, of which, however, I have not been able to obtain a copy, were published in 1855. Dr. Thomas, of New York, informs us that Dr. Marian Sims introduced the practice of thus employing fluid caustics in the treatment of intra-uterine disease into the woman's hospital in that city with great advantage, and as Dr. Sims left America in the year 1862, he must have carried out the practice some time prior to that date.

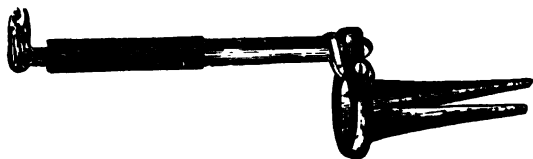
There can be no doubt, however, but that the first person who ventured to apply the strong nitric acid to the cavity of the uterus in this country was Dr. Kidd. Dr. Marian Sims's method of applying fluid caustic to the interior of the uterus, as described by Dr. Thomas, is, "To wrap a thin film of cotton round a probe, and, having dipped it into the fluid selected, to pass it up to the fundus of the uterus, and to keep it there for about half a minute." This, in the main, is identical with the plan advocated by Dr. Miller. This treatment is simple, safe, and painless, but, if carried out on the above method, the cauterization thus effected is not in general sufficient; the cervix and os internum are doubtless in all these cases relaxed and patulous, but not sufficiently so to prevent their pressing out much of the caustic with which the *film* of cotton is saturated, and if the uterus be, as is not unfrequently the case, retroflected or antelected, it is hardly possible that any caustic will remain on the cotton when the probe has reached the fundus. It is therefore most desirable first to dilate the cervix still further by the introduction of one or two pieces of sea-tangle or sponge tent, and, when the application is about to be made, to seize the anterior lip of the uterus with a vulsellum, so as to bring it low and at the same time to steady and straighten the organ; you can now pass a stilette armed with a comparatively thick layer of cotton or roll of lint rapidly up to the fundus; indeed, if the case be severe or of long standing, this should be done twice, so as to

insure the thorough cauterization of the whole inner surface of the uterus. I have practised this method frequently with great success, and in no one instance have seen any unpleasant results, though, at the same time, I freely admit that a cure does not invariably follow.

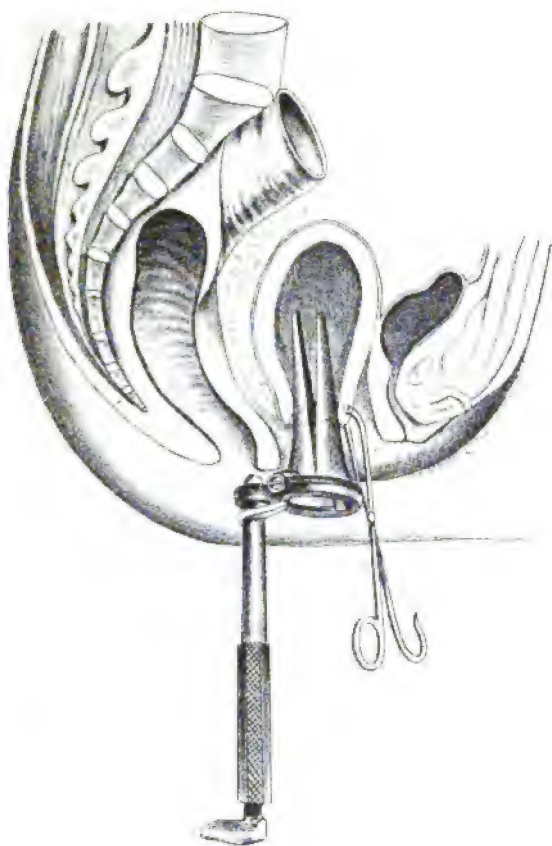
I have been thus minute in describing the method of carrying out this practice because some practitioners have still a great dread of applying powerful caustics to the interior of the uterus—a fear which is totally groundless. Applying strong nitric acid, in the manner described, to the cavity of the uterus, seldom causes any pain whatever. And in this respect its application differs entirely from that following the injection of even weak solutions of caustic into the uterus; grave symptoms, and even death, having followed that practice. Therefore, while I advocate the use of nitric acid and of the solid nitrate of silver as safe applications to the interior of the uterus, I strongly object to intra-uterine injection of any fluid in the treatment of the class of cases under consideration.

There is just one precaution needed in carrying out this practice, and that is to guard the cervix uteri, in cases where the cervical canal is healthy, from the action of the nitric acid, for if this precaution be neglected, contraction of that canal is likely to occur.

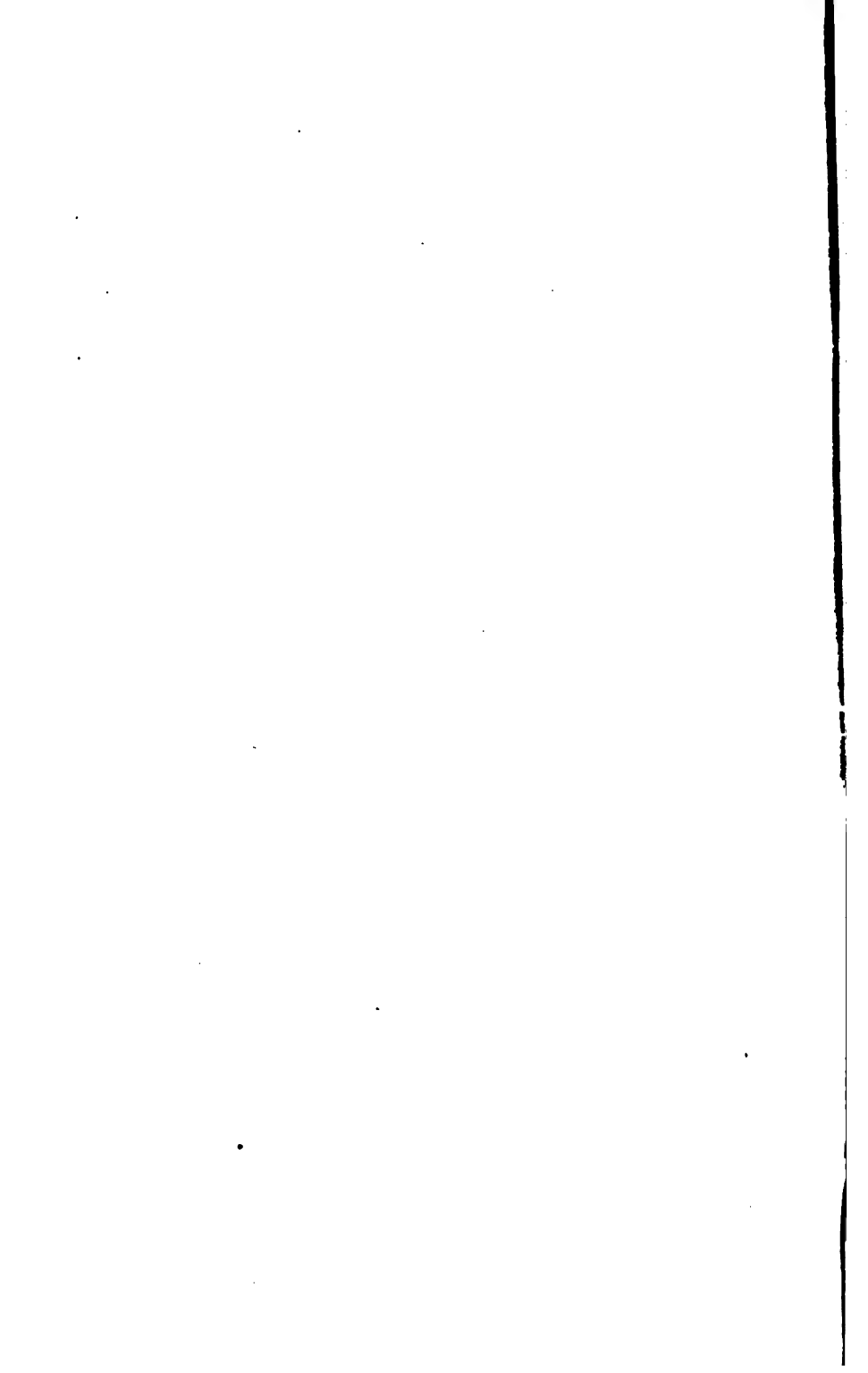
But the mode of applying nitric acid which I have described, and which is that adopted by others in this city, and until recently by myself, is liable to this objection, that it cauterized most freely the lower segment of the cervix, the part which in general requires it least, and which in many cases it is desirable should escape entirely. I have, therefore, devised this little instrument, which I may term an *intra-uterine speculum*;



it is, in fact, something like an aural speculum, only that it is expanded by means of a screw which works through a long handle. I have tried it, and it answers the purpose in view very well; namely, the protecting the cervical canal from the action of the acid, and the ensuring the thorough cauterization of the cavity of the uterus. It is only intended to be used after the cervix has been dilated, and not as a dilating agent. When it is employed, the duck-bill speculum may be withdrawn, as both the cervix and vagina are protected from the action of the acid; but it is not advisable to release the exterior lip of the uterus from the grasp of the vulsellum, as, were this done, the speculum might slip out of the cervix. The instrument is made of vulcanite, by Arnold & Sons, of West Smithfield, London. It is possible that one of platina could be made

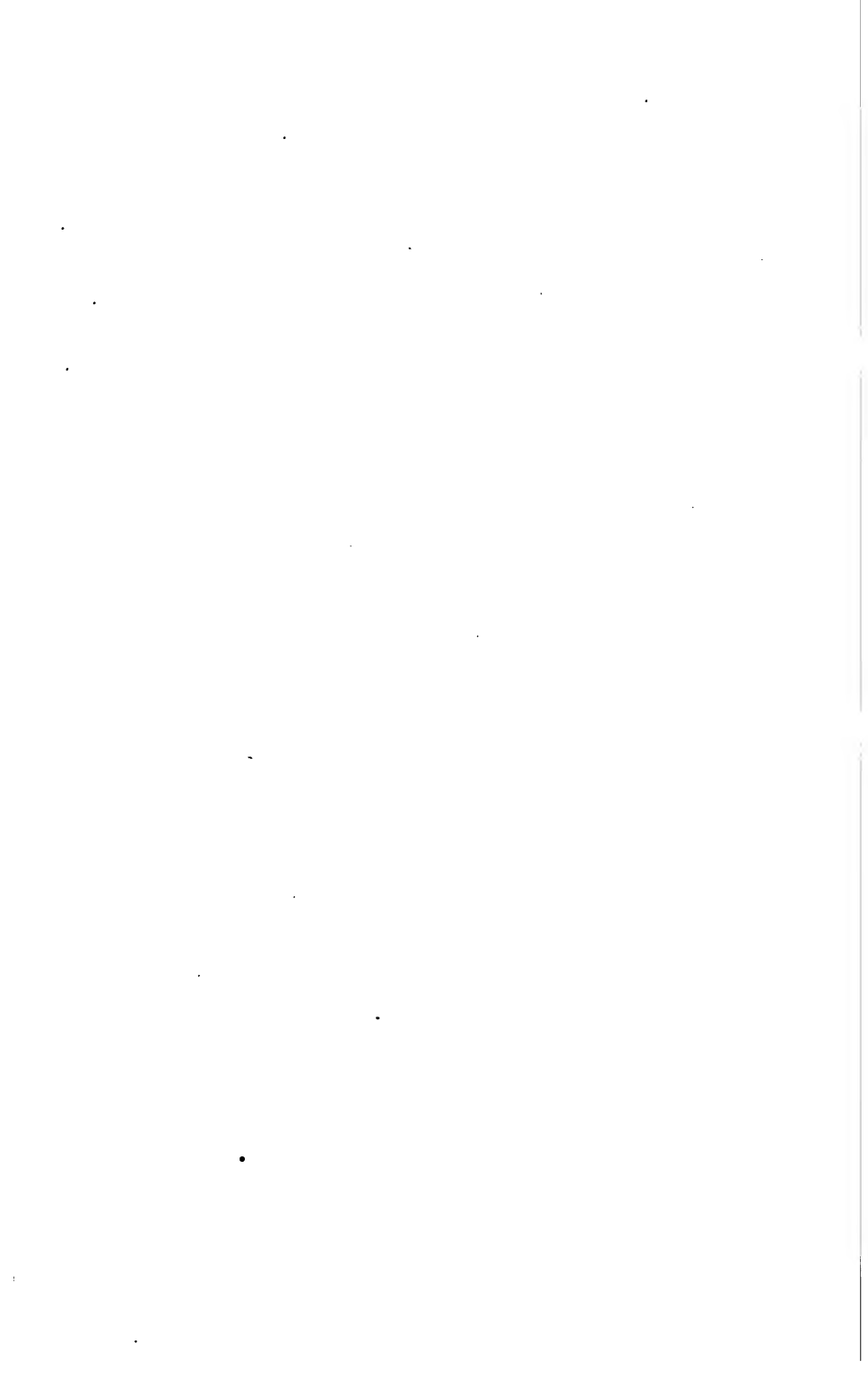


Intra-uterine Speculum *in situ*.



smaller and equally serviceable, but the expense of such an instrument would be considerable. I believe this instrument will prove of value in facilitating the treatment of intra-uterine disease.

Of numerous cases of endo-metritis, which I thus treated, I shall give very briefly the details of three. The first was evidently totally unconnected with pregnancy, her last child having been born twenty years previous to her coming under my care. She had also been a long time a widow. Ten years ago menstruation suddenly ceased, and she suffered from headache and dizziness. These symptoms yielded to treatment, and menstruation again became normal. Four years elapsed and then the periods began to become more profuse, were attended with very severe pain, and occasionally clots were expelled, their passage being followed with but little relief. Leucorrhœa was present during the interval between the periods. She also suffered from constant pain in the left side, felt most intensely at a point midway between the spine and crest of the ilium. This pain, at first experienced only at each menstrual period, became, after a time, constant, being aggravated in intensity during the periods, sometimes, indeed, becoming at those times absolutely intolerable; there was also tenderness over the right ovary. The uterus was tender to the touch, was enlarged and retroflected. The introduction of the sound caused much pain and some blood followed its withdrawal. The cervix, was swollen and much engorged. To relieve this condition I punctured it, it bled freely, and, hoping to lessen the ovarian congestion, I directed 15 grains of the bromide of potassium to be taken thrice daily; this treatment was continued in for some time; blood being extracted locally at intervals of five days. The result was that the cervical engorgement was removed, menstruation became somewhat less profuse, and the ovarian pain much mitigated in severity; but treatment having been discontinued for a short time, the whole train of bad symptoms returned, and I became convinced that no permanent relief would be obtained unless I treated the interior of the uterus directly. I accordingly explained my views as to the nature of her case to this lady and to her son, himself a surgeon. She consented to undergo any treatment which promised relief from her sufferings. I commenced by introducing five pieces of sea-tangle bougie into the uterus; these dilated the cervical canal so freely that I passed my finger through the os internum and up to the fundus of the uterus; as I had anticipated, I detected a rough granular condition of its lining membrane, the lip of the uterus was then seized with a vulsellum and drawn down, and a wire armed with a roll of cotton thoroughly saturated with the fuming nitric acid, was passed up to the fundus and retained there for some seconds; this was done twice so as to secure a thorough cauterization of the whole interior of the uterus. I had the advantage of Dr. Denham's assistance in this case, and he can



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testify to the thorough manner in which the cauterization was performed. The patient was not chloroformed as she objected to being so. No pain followed. I kept this lady in bed for some days as a precaution, but no other treatment was adopted. The next period came on a little before its time and was profuse, but attended with less pain than previously. Since then her condition has steadily improved, the periods now last but three or four days, and are almost painless. This lady had been treated in various ways, without benefit, before she came under my care. I may here remark that if the nitric acid be applied shortly before a menstrual period, that period is likely to be profuse, but this by no means indicates that the treatment is a failure, the subsequent ones, as in the present instance, frequently becoming normal.

The next is that of Mrs. —, aged twenty-six. She was confined prematurely of her first child in December, 1870. She was not able to nurse, and recovered very slowly. For some months subsequently she menstruated at regular intervals, the flow not being at first excessive. She suffered, however, constantly from leucorrhœa which debilitated her greatly; for this she was treated by the exhibition of tonics and by vaginal injections without benefit. In the July following her confinement she observed that the menstrual period was not only greatly prolonged, but that the quantity of blood lost was excessive. From this date till the month of October, menstruation recurred regularly at intervals of but fourteen days, the flow lasting very profusely for seven days. The period which occurred immediately before my seeing her, commenced on the 18th October and lasted for a fortnight; on the fifth day of the flow, after having been, unfortunately, obliged to pass the greatest part of the night in attendance on an invalid, she was attacked with severe pain which she referred to a point immediately above the pubes; this pain was very intense, and had not entirely subsided when I saw her.

On making an examination I found that the uterus was very low in the pelvis; the cervix was soft, tumified, and tender to the touch; the fundus was evidently enlarged, anteflexed to a considerable degree, and very sensitive, the slightest pressure causing great pain. The sound passed with ease to the depth of three and a half inches, but on pressing the point against the fundus she immediately complained of the great pain she experienced. The cervix when exposed to view was seen to be in a state of extreme congestion, while a copious glairy discharge issued from the os uteri.

The diagnosis was clear; an attack of acute endo-metritis had occurred in a uterus which was in a state of sub-involution, the acute attack having now passed into a chronic form; endo-cervicitis was also present. Puncturing the cervix was, in this case, productive of marked relief. The endo-cervical discharge became much less copious, and the tenderness

on pressure was greatly diminished, and vaginal aspect of the cervix assumed its normal appearance; yet menstruation continued profuse, and was attended with great pain. This patient resided some distance from town, and I would not venture to dilate the cervix in a patient whom I could not watch. I accordingly, several times, used the nitric acid in the mode recommended by Dr. Sims and Dr. Thomas, but no improvement followed. I, therefore, decided to introduce ten grains of the solid nitrate of silver up to the fundus, and leave it there to dissolve. This I accordingly did. A good deal of pain followed the application, but it subsided in a few hours, and the patient rapidly improved. Menstruation became normal, and now, after the lapse of a year, she continues quite well.

In the last case which I shall allude to the symptoms were in many respects very different from either of the former. The patient had borne six children, the last nearly ten years prior to her coming under observation. Ever since its birth she had suffered from menorrhagia, with profuse leucorrhœa, in the intervals between the periods. Of late the flow had become very excessive, lasting for sixteen or seventeen days; but, exhausting as this continuous drain was, she complained even more of irritation of the bladder, which compelled her even at night to micturate every hour, or at most every two hours, and during the day the desire to do so was constantly experienced. In her the vaginal aspect of the cervix and the cervical canal were perfectly healthy, but the fundus was enlarged, globular in shape, and its cavity elongated.

The diagnosis was, in this case, obscure; the menorrhagia could be accounted for by the presence of an intra-uterine tumour. The excessive vesical irritation, manifested by the incessant desire to micturate, was not, however, a usual accompaniment of the latter affection. A correct diagnosis could only be arrived at by a digital examination of the interior of the uterus. To effect this the cervix was fully dilated; it disproved the existence of any tumour, but detected a roughened granular condition of the lining membrane of the body.

In this case, also, I cauterized the whole interior of the uterus very freely with the fuming nitric acid; no pain followed, nor was any special treatment subsequently needed. The night following the day on which the operation was performed this patient slept soundly, without an opiate, and for the first time for years was not disturbed by the desire to micturate. I had a letter from this poor woman recently, and she speaks with delight of the comfort she now experiences. This case is interesting in proving not only how obscure may be the symptoms, and how severe the sufferings occasioned by disease of the mucous membrane lining the cavity of the uterus, but also for how long a period these cases continue.

In the last case the particulars of which I have detailed while irritation

of the bladder and menorrhagia were constantly present, there was no cervical inflammation, and the uterus itself was but little tender to the touch. I therefore did not practice any preliminary treatment, but proceeded at once to explore the interior of the uterus, and finding that the mucous membrane lining it was in a granular condition, I applied the fuming nitric acid freely, with the most satisfactory results.

I desire to impress on the Society the fact that nitric acid properly and carefully applied to the interior of the uterus is a perfectly safe and painless and at the same time efficient application. I do this the more emphatically, because a writer in the number of the *Edinburgh Medical Journal*, for February, 1872, thus expresses himself with reference to the subject:—

“Patients treated as he (Dr. Atthill) suggests, will certainly have no more menorrhagia, they may be glad if they have even a uterine cavity left.” And again, a little further on—“The treatment is one which may have been tried, but which we are pretty sure has never been carried out.”

Now with respect to the last of these assertions I have only to state, that in common with many other members of this Society, I have repeatedly applied the fuming nitric in the manner described, not only in the presence of other practitioners, but also of a large class of pupils. In the majority of these cases the cervix and os internum had previously been so freely dilated as to permit the passage of the index finger up to the fundus, for the purpose of exploring the interior of the uterus. It is also commonly the practice in this city to apply the fuming nitric acid to the cavity of the uterus, after the removal of intra-uterine tumours, the cervix being sometimes so fully dilated as, in one of my own cases, to have permitted the extraction of a polypus as large as a turkey egg. In all these cases the os was not only exposed to view by means of the duck-bill speculum, but the uterus was drawn down and firmly held by means of a vulsellum fixed in the anterior lip. Under such circumstances there can be no doubt but that the interior of the uterus was freely cauterized, that the uterine cavity remained was subsequently proved by examination and that it had been restored to health, was evinced not only by the fact that the distressing symptoms under which the patients laboured had been removed, but further and in an unequivocal manner by pregnancy having in some cases followed. It is needless to say more on this subject, except to express regret that a writer in a medical journal, of high repute, should express decided opinions on subjects of which he is plainly ignorant. I have seen pelvic cellulitis follow the application of the liquor of the perchloride of iron to the cervical canal, but I have not ever seen an unpleasant symptom follow the use of the nitric acid.

In conclusion, to guard against misapprehension, I think it right to

add that, in advocating this method of treating endo-metritis, I must be understood to refer only to cases in which hæmorrhagia, purulent discharges, or profuse uterine leucorrhœa exist, or to cases in which other means have, on a full and fair trial, failed to effect a cure.

DR. CHURCHILL said he rose to express his sense of the value of Dr. Atthill's paper. He believed that our recent knowledge of the pathology of the uterine cavity was the greatest advance that had been made of late years.

It would be within the recollection of many that he (Dr. C.) had read before this Society, and afterwards before the Medical Association, a paper on granular inflammation of the cervical canal. Since that time the use of multiple tangle tents, for which he was indebted to Dr. Kidd, had enabled us to examine not only the cervical canal, by the endoscope, but the entire cavity of the uterus. In one such case at the Rotunda Hospital the rectum tube of the endoscope was passed to the fundus, and every portion of the cavity plainly seen. He had found general redness, or redness in patches, with or without the glistening granular appearance which he described as seen in the cervical canal.

He could bear testimony to the value of the free use of strong nitric acid. It neither gives pain, nor is followed by unpleasant consequences, perhaps because it does not make a deep slough. He had been using it freely for the cervical canal for a great many years, and for the entire cavity for some time.

DR. RINGLAND said the Society was greatly indebted to Dr. Atthill for his able paper, in which he had given so full and complete a history of the symptoms and the treatment of this formidable disease. He thought the last observation he had made, in commenting on a review that appeared in a Medical Journal, on his (Dr. Atthill's) admirable papers on this subject, was a correct one, viz., that the review in question was the production of a gentleman commenting on a mode of treatment of which he had no experience. He fully endorsed what Dr. Atthill had said of the introduction of solid caustic into the cavity of the uterus. As to the application of nitric acid to the interior of the uterus, he was not aware until lately that Dr. Marian Sims had been in the habit of using it; in fact, he thought that he (Dr. Ringland) himself was the first to suggest the use of it. A paper was communicated some years ago to the Society by Dr. Kidd on the removal of intra-uterine polypi, and he detailed a case where they occurred again and again, necessitating frequent operations for their removal, and accompanied on each occasion with great hæmorrhage. On consultation he (Dr. Ringland) suggested the use of nitric acid in that case to check their growth. He had been in

the habit of applying nitric acid to the cervix, and he had known of its application in the case of hæmorrhoids, with the result of inducing a healthy action in the vessel itself. Seeing a large amount of hæmorrhage in cases of cancer of the uterus, it seemed to him that the same thing might be applied there, and that solidification of the vessels of the uterus might be effected. The result proved that he was right. It struck him that hæmorrhage in the cavity of the uterus, resulting from polypi or other causes, might be effectually controlled by the same means, and he suggested the application of nitric acid to Dr. Kidd and the late Dr. Beatty. They seized upon it, and he thought the paper read by Dr. Atthill showed that it was not altogether an unimportant or unwise suggestion.

DR. DENHAM said he had the pleasure, some twenty-five years ago, of seeing the President (Dr. Kennedy), applying nitric acid in a case of hæmorrhage, and he thought therefore Dublin Obstetricians ought not to be too modest in allowing their American cousins to carry away all the honours. He remembered being astonished at the time at seeing that strong caustic put into the cavity of the uterus, but experience had proved that it was a most valuable remedy. In the Rotunda Lying-in Hospital nitric acid was used with great advantage and without bad consequences. In one patient it failed to produce an effect, and he then threw up by an india-rubber bag a strong solution of iron with a most satisfactory result. He could fully testify as to the excellent effect of nitric acid in one of the cases mentioned in which he had assisted Dr. Atthill.

DR. JAMES LITTLE said he could testify to the great facility with which Dr. Atthill's intra-uterine speculum was introduced, and the safe way in which applications were carried into the womb through it.

DR. KIDD fully concurred in the remarks which Dr. Atthill had made as to the importance of acquiring a real knowledge of the diseased condition he had described, and of the method of treatment. There were no cases more difficult of recognition than those of endo-metritis, none more difficult to manage, and none more productive of bad health and distress. With regard to the application of nitric acid, Dr. Ringland had originally proposed it to him; he confessed he was startled at the suggestion, and it was only after some consideration he adopted it. It was then new to him to apply it to the cavity of the uterus; however, there was nothing new under the sun. Dr. Denham spoke of seeing it applied twenty-five years ago; but Ambrose Parè had described it fully as a remedy for warty growths from the interior of the uterus.

DR. J. A. BYRNE expressed his full approbation of the line of treatment Dr. Atthill had advocated. He thought it was one seldom attended

with danger. Although nitric acid would appear at first to be a very severe remedy, yet after all, the wound it produced was a very superficial one, and the amount of suffering caused was not great. They knew this by its application to syphilitic ulcers; and it would appear that it did not produce a slough, but some alterative effect on the surface of the uterus. He fully concurred with Dr. Atthill, and joined him in condemning statements which, if not refuted, might have produced a very injurious effect in preventing the application of this valuable remedy.

The PRESIDENT said he took the same view as Dr. Byrne of nitric acid, namely, that it sounded more severe than it really was. It acted over a large extent of surface, did not go very deep, and produced a very healthy sore underneath. His attention was first called to it with a view of dealing with malignant disease, and he carried it out very largely, certainly before 1846. When he first made the application he was somewhat apprehensive as to what the result might be; but there never was hæmorrhage, never any inflammatory action, and he never found any serious result from it. In some cases, especially those equivocal phagedenic cases, he had found it to act most satisfactorily, the result being that a healthy action was set up. He had used nitric acid in the interior of the uterus, and had never seen any unfavourable result. He did not believe the interior of the uterus took on adhesive inflammation, nor had he ever seen cellulitis produced as the result of the application of nitric acid.

The abstraction of blood has been alluded to. He did not think sufficient attention was given to this subject. His own experience of the use of scarification was disappointing. He rarely could get a proper quantity of blood, but in some cases he got too much. They could, however, always depend on leeches, but these should never be applied unless "harnessed." He would advise that no more than two leeches should be applied at a time.

DR. ATTHILL thanked the Society for the kindness with which they had received his paper. He should not have brought it forward except for the importance of the subject, and also to reply to the strictures which had been passed upon this method of treatment without an adequate knowledge of the subject. He thought the President was perfectly right in saying there was no danger of adhesive inflammation in the body of the uterus from the use of nitric acid, but that there was danger of it in the cervix. Therefore, if the cervix was healthy they should save the canal, and it was with the view of protecting it that he had devised the instrument which he had exhibited. He hoped no one would suppose that he advocated cauterization in all these cases. He had distinctly stated that when urgent symptoms were not present, general treatment should

first be fully and fairly tried ; but when the disease was of old standing, constitutional treatment was useless. As to scarification, he had tried it and it was useless ; but puncturing the cervix in the manner suggested by Dr. Hall, of Brighton, was generally very satisfactory. He had treated many cases by simple puncture, and got by that means as much blood as he wanted.

The PRESIDENT observed that he had repeatedly seen adhesion of the os, but never of the uterus itself.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

ROBERT W. SMITH, M.D., Secretary.

MR. TUFNELL, President, in the Chair.

Cirrhosis of the Lung.—DR. EAMES said the lungs which he now exhibited were taken from the body of a man who died in Mercer's Hospital on the 3rd of April last. He was admitted on February the 13th. He was aged thirty-two, and earned his livelihood as a painter. Fourteen months previously he got a cold and had some cough. This continued and grew worse, and was especially bad at night; it was, however, continuous during the day. He lost flesh continually, and especially in the limbs. He had a copious, whitish and sometimes yellowish expectoration, but he never at any time during his life spat a drop of blood; he stated positively that he never had any hæmoptysis or hæmorrhage of any kind or description. He was seen by Dr. Eames in August last, in a casual way, when he found that he had resistance on pressure under the left clavicle; he had some increase of vocal fremitus and some depression under the left clavicle, and also some dulness. He diagnosed the case as one of phthisis, and dismissed it as such. The man came back to hospital on February the 13th, 1872. His body was not as much wasted as one would expect in a man who had pulmonary phthisis. His limbs, however, were considerably wasted; he had pallor of the surface, and the skin had a satiny feel when the finger was passed over it; he had also clubbing of the fingers and toes. The left side was smaller than the right. The latter measured $17\frac{1}{2}$ inches, whilst the left side measured only 15 inches. When he took a full inspiration the left side did not expand as the other did, but was lifted up by the inspiratory muscles. There was absolute dulness over the entire of the left side, but the right side was perfectly clear and its percussion clearness passed across the sternum to the left costal cartilages. On the left side there was no vesicular murmur whatever, but rhonchi and râles were heard scattered through the lung and there was tubular breathing. Over the right side there were rhonchi too, but also, very strong puerile respiration. The heart's apex was displaced fully an inch outside the left nipple, and half an inch below it. The man got bronchitis and died. On making the *sectio cadaveris* in the usual way, on raising the sternum the entire thorax seemed to be occupied by the right lung—the left lung was not at all seen, and the

heart was pushed out of its place. On groping back into the left side, the left lung was found in the condition as now exhibited. It had to be cut and torn out of the chest. It was dark red in colour, with bluish mottling through it, fibrous and hard. There was to be seen here and there through it dilated bronchi and that tubercular structure found associated with cirrhosis of the lung. There were some crude tubercles at the apex, which also had been found in some cases of this disease. The left pulmonary artery was very small, whilst the right was enlarged. He thought this case presented a good example of cirrhosis of the lung; there was enormous thickening of the pleural membranes, the thickening measuring in some places $\frac{3}{4}$ ths of an inch. The right lung was greatly enlarged, and free from tubercle. Its great size accounted for the clearness on percussion transgressing the mesial line, and it was also in a generally emphysematous condition.—*April 13, 1872.*

Amputated Genital Organs.—DR. BARTON exhibited the penis, scrotum, and testicles of a man, who inflicted the injury upon himself of removing them, while in a state of great mental depression. He was aged forty-eight, and was a butler residing with a gentleman in the country. He was a married man and had a family. He unhappily became entrapped into a clandestine intercourse with the housekeeper. He subsequently confessed this to his wife; but the resolutions then made were of no avail, and the illicit connexion was renewed. This appeared to prey upon his mind and to render him miserable.

Upon the 28th of last month, when in a state of profound depression he retired to his pantry, took a bread knife, and seizing his penis and scrotum, he, with one stroke, amputated them, from behind forward. He was found in the pantry weltering in his blood. Dr. Halpin, of Arklow, when called in, found that the bleeding had been arrested, and no ligature was required. He drew the wound together with sutures, put a catheter into the urethra, which was amputated close to the pubis, and brought the man up to the Adelaide Hospital. He was found to be in an extremely desponding state of mind, continually bemoaning his condition, and wondering any one would pay any attention to him. The first remarkable change observed in him was on the 1st of April, three days after his admission, when severe rigors set in, and continued to recur daily, along with uncontrollable hiccough.

On the 12th of April he was reported as pretty well; the pulse seemed stronger, the appetite was very good, and he slept all day. On the 13th he was very restless, and on the evening of the same day the physical signs were, for the first time, discovered of any pulmonary affection. There was dulness on percussion over the lower half of the right lung, and over the greater part of it crepitating râles were heard. He died on

the following morning (the pulse having become extremely rapid), seventeen days after the infliction of the injury.

It was found on *post-mortem* examination that half of the right pleura was filled with purulent matter, and the lung on the right side had small purulent deposits in it. The liver was enlarged, but there were no purulent deposits. The kidneys were in the first stage of albuminous degeneration.

In addition to these visceral changes a small collection of pus was found in each lateral lobe of the prostrate gland; a considerable quantity was also contained in the bladder. The wound was three inches in length, and no unhealthy action had taken place in it. The cause of death was, evidently, pyæmia. How far the state of mind observable in this man, as in all similar cases, conduced to this result, is an interesting question.—April 20, 1872.

Extreme Contraction of the Tricuspid and Mitral Orifices unattended by presystolic Murmur.—DR. CRYAN exhibited the heart in this case, and said that the patient, Mary R., a dressmaker, aged twenty-seven, unmarried, and of temperate habits, was admitted into St. Vincent's Hospital under his care, on the 27th of March, 1872, labouring under many of the symptoms of advanced cardiac disease. She was a woman of short stature, slight and thin, with prominent, congested eyes, and large pupils; her face was very livid, and its expression anxious and careworn.

She said that she had generally enjoyed excellent health until six years ago, when she was confined to bed for a month with rheumatic fever, during the course of which she was not aware of having had any symptom of heart affection; that in two months she had recovered her usual strength, and, subsequently, remained quite well for about two years, when she began to suffer from palpitations of the heart, and shortness of breath after even slight exertion; and that nine months ago, she was affected in addition, with cough, and spat a tea-spoonful of blood on two occasions, and noticed, for the first time, some swelling about her ankles in the evening. There was no family history of heart disease.

State on admission.—She suffered from debility, dyspnœa, short cough, with scanty sero-mucous expectoration, and complained of a painful sensation of weight and distension about the margins of the right false ribs, below which the smooth liver projected fully two inches; there was no ascites, but the legs were cedematous, pale, and cold; she had no appetite, bowels regular, urine scanty, tongue clean, and moist; catamenia absent four months. The impulse of the heart was strong, jarring, and extended; the action of the organ slightly irregular; the pulse at the wrist very small, weak, and irregular, 90 in the minute, its weakness contrasted strongly with the strength of the cardiac impulse; but there was no

difference to be found between the rate of the pulsations of the heart and of those of the radial artery at the wrist; respirations shallow, 36 per minute.

The area of precordial dulness is much increased to the right and inferiorly, and there is visible pulsation in the epigastrium; the apex beat is situated between the fifth and sixth ribs, nearly one inch internal to the left nipple; there is no purring thrill, or *fremissement* perceptible over any part of the heart; at the left apex there is a soft blowing ventricular systolic murmur, lost near the base of the heart, audible in the axilla, but not posteriorly; this murmur, which replaces, or quite masks, the normal first sound over the left ventricle, is loudest immediately above and to the outer side of the apex, and is plainly synchronous with the pulse in the carotid artery. There is no murmur at the base of the heart, or over the lower end of the sternum, or the sternal end of the fourth right intercostal space. At the second right cartilage, the sounds are feeble in the aorta; on the other hand, over the pulmonary artery, both sounds are loud, the second, especially, being much intensified, not merely relatively, but absolutely—the percussion note over the second left costal cartilage is quite normal. The external jugular veins are dilated and turgid, the right slightly knotted, or varicose, but they are not pulsatile; the internal jugulars exhibit, for two inches above the clavicles, visible pulsations *synchronous with the ventricular contractions*. There is no visible pulsation of the arteries; no murmur in the vessels of the neck. There is want of resonance on percussion over the lower lobe of each lung posteriorly, and small mucous râles are there audible. The diagnosis announced was:—"Mitral insufficiency, combined, in *all probability*, with contraction of the mitral orifice; tricuspid regurgitation, due probably to *dilatation* of the right auriculo-ventricular opening; hypertrophied and dilated heart; great congestion of the lungs and liver."

On the 28th of March, the day after admission to hospital, the report goes on to state that the patient has had a few hours' sleep last night; the palpitations, dyspnoea, and cough are less distressing, but she complains much of "noises in her head;" the urine is very scanty—about half a pint during the last twenty-four hours. It is loaded with urates, but not albuminous; bowels free, no thirst; the heart's action is less irregular and its impulse is stronger; no presystolic murmur or purring thrill; the ventricular systolic bruit has its maximum intensity over the left apex, and is quite audible in the left axilla, but not at all at the lower angle of the scapula; the radial pulse continues small, weak, and irregular. For the next twelve days the patient's strength was gradually failing, and she suffered from occasional attacks of cardiac asthma; the physical signs presented no remarkable change. On the 10th of October the dyspnoea and cough were more distressing, and the systolic murmur was,

at times, quite inaudible, and the pulsations in the radial arteries were not to be felt; there was no œdema of the upper extremities, and the impulse of the heart was moderately strong, but its action very irregular.

This morning, April 11th, after a sleepless night, she was seized with severe dyspnœa, and complained of great oppression about the heart; the cardiac impulse was extremely feeble, the action irregular, with frequent intermission in the beats, and the sounds of the organ were almost inaudible; there was some increase of the precordial dulness, but the weak state of the patient necessarily prohibited any very accurate examination; the radial pulses, however, as on the preceding day, are not to be felt, but there is feeble pulsation in the brachial arteries; her mind continues remarkably clear and intelligent; she now grew weaker every hour, and in the evening passed into a state of syncope, in which she died in the course of the night.

On *post-mortem* examination, twelve hours after death, the pericardium, which was free from adhesions or other signs of inflammation, was found to contain about half a pint of clear, pale, yellowish serum; the heart was globular in shape, and weighed eleven ounces; its muscular tissue was red and firm, and, on microscopical examination, presented no sign of fatty degeneration; the left ventricle was of natural size, neither dilated nor hypertrophied, its cavity was quite empty; the left auricle was dilated to about twice its normal size, and its walls were hypertrophied, measuring in parts one-fifth of an inch; its cavity contained a few red coagula, and its lining membrane was slightly thickened, and of a pale buff colour; the pulmonary veins were dilated to twice their usual dimensions; the mitral orifice was so contracted that the point of the little finger would not enter it; when viewed from the auricle, its figure was crescentic, with the margins hard and rough; the mitral valve was shortened, contracted, and quite calcareous, with its ventricular orifice oval, and rigidly patent; the primary chordæ tendinæ, or those arising from the papillary muscles, were shortened and thickened, and those of the right bundle had for the most part coalesced; the *secondary* chordæ tendinæ had for the most part disappeared, as well as the thin web-like edge of the mitral valve into which, in health, they are inserted; the two papillary muscles were not altered in size or structure; the aortic valves were found to be competent, when tested by the water-pipe—the posterior one was opaque and a little thickened; the coats of the aorta were healthy, but its calibre was diminished about one-third; the right ventricle was moderately dilated and hypertrophied; its anterior wall, near the base, was three lines thick; the tricuspid orifice and valve were so much contracted as to admit only the index finger; the cusps of the valve were opaque and a little thickened, but not calcareous; the muscoli papillares were somewhat increased in size; the pulmonary artery was

hypertrophied and dilated—its circumference measured four inches; the pulmonic valves were healthy; the right auricle was a little hypertrophied and its cavity, dilated to nearly twice its natural size, was like that of the right ventricle, more than half full of dark-red *post-mortem* clots. The lungs were greatly congested.

Dr. Cryan thought that the case just narrated justified him in drawing the following conclusions:—

1. That the extreme contraction of *both* auriculo-ventricular orifices may be present in the *same* patient, and yet be unattended during life by any *presystolic* murmur.
2. That as the impulse and contractions of the heart continued strong and vigorous for twelve days, out of the fifteen that the case was under observation in hospital, the absence of *presystolic* murmur during that time must be ascribed—at least in the case of the mitral orifice—to some other cause than mere weakness of the auricular contractions.
3. That as the mitral orifice was found, at the autopsy, not to be smooth, the absence of *presystolic* murmur in this case was probably due to the smallness of the blood-current transmitted through the greatly contracted auriculo-ventricular opening.
4. That when in mitral regurgitation, of long-standing, the apex beat is not displaced *downwards*, and the other signs of dilated and hypertrophied *left* ventricle are absent—the pulse at the wrist being very small and weak, while the impulse of the hypertrophied right ventricle continues strong—we may, even in the absence of *presystolic* bruit, infer, with much probability, that there is also considerable *contraction* of the mitral orifice.—*April 20, 1872.*

Intracranial Tumour; Glioma of the Fifth Nerve.—DR. T. E. LITTLE laid on the table a specimen of a remarkable form of tumour, which engaged several important structures at the base of the brain.

It had occurred in the person of a man, of the age of forty-six, who had been an extremely intemperate liver, and who had, some ten or twelve years ago, suffered from a severe and protracted attack of constitutional syphilis; his wife had aborted three times in consequence of this disease, communicated to her.

The first symptom of his disease occurred about twelve months before he died. This symptom was intense pain throughout the district of the branchings of the fifth nerve of the left side. This pain gradually became more and more excruciating, and eventually came to be almost continuous except when relieved by narcotics. Last December (three months before death) he became suddenly paralysed of the affected (left) side of the face; about the same time he perceived that he had double vision, and soon afterwards he began to lose the sight of his left eye. He was an

inmate of Sir Patrick Dun's Hospital in the month of February, 1872, and at this period the following was, briefly, his state:—He still suffered much from deep-seated pain of the left side of the face; and it was chiefly for this, and for the condition of blindness of the left eye he sought relief. Paralysis of several of the cranial nerves existed, of which the following are the details:—There was partial paralysis of the left third nerve, complete paralysis of the left sixth nerve, complete paralysis of the portio dura, with partial paralysis of the portio mollis of the left seventh nerve, complete paralysis of the left fifth nerve—motor, sensory, and gustatory; in addition to these symptoms, a large opaque ulcer occupied the lower two-thirds of the left cornea, and there were evidences of deep-seated involvement of the left eye-ball. Shortly after this the paralysis of the third nerve became complete, and he became absolutely deaf to all sounds (the deafness of the right side, however, was an old and unconnected circumstance). In this condition he died, retaining his consciousness to the last.

On opening the head, the brain generally and its membranes were found to be unusually anæmic. A tumour of irregular shape, and considerable size, was found at the base of the brain, in the following situation. The principal bulk of it existed as a flattened mass, involving and lying in the left side of the tentorium; it thus lay between the middle lobe of the cerebrum and the cerebellum. From this it extended outwards for some distance into the dura mater, covering the upper surface of the petrous bone, and forwards into the cavernous sinus, the posterior part of which it almost completely blocked up, where it involved the Casserian ganglion, and the nerves passing into this space. A small nodular piece, almost separate from the larger mass described, and about as large as a small nut, took the place of the part of the fifth nerve, between its apparent origin and the ganglion, and encroached upon the pons Varolii at the place of origin of that nerve, where a small softened excavation existed in that structure. The diseased mass did not project extracranially in any place.

A considerable region of the posterior part of the middle lobe of the left cerebral hemisphere was far advanced in a state of softening. The petrous bone, in the region of the tumour, was roughened and eroded to a slight degree.

Drs. Yeo and Harvey (who undertook the microscopic examination of the tumour), made the following communication as their report:—

"The tumour consists of a delicate network, containing within its meshes various cell forms, with some nerve fibres passing through it.

"Sections, with the cells shaken out, show the network to be composed of an interlacement of delicate bands, resembling the neuroglia of Virchow; thickened, however, and more granular. In some places

it seemed to be composed of anastomosing, multicaudate cells; in others, the bands presented an irregular, or even fibrillar appearance. Some of the multicaudate cells, broken off from the reticula, contained distinct nuclei, and gave the idea of being in a condition of rapid proliferation. The greater number of the cellular elements resemble those of the ordinary small-celled sarcoma. There exist, also, numerous spindle-shaped cells of different sizes, some short and obtuse, others very much longer, presenting various gradations from the ordinary cells to the multicaudate cells mentioned above.

"The fine nerve bundles which run through the tumour are widely separated by the proliferating neuroglia, and the nerve fibres are studded with numerous nuclei.

"We are of opinion that this is a very well-marked example of the class of tumours called by Virchow 'Glioma,' commencing in the nerve tissue.

"G. F. YEO,

"R. J. HARVEY."

April 27, 1872.

Hæmatocele of very long duration—Suppuration of the Sac from injury—Castration.—DR. BARTON said: I removed this morning the tumour now on the table by the operation of castration. It seems to me to present several points of pathological and practical interest. The patient was a man, aged fifty, a farm-labourer and carter. About thirty years ago he got a kick from a horse in the left testicle; a swelling followed, which remained ever since much the same, being perfectly painless. He sought no advice regarding it. He married, and became the father of a family, the testicle remaining the same, viz., the size of a large orange. About the beginning of July, 1871, while standing upon the wheel of his cart, unloading potatoes, his foot slipped, and he fell across the wheel, the lower part of the abdomen coming in contact with the iron tire. Congratulating himself that his old swollen testicle had so narrowly escaped the blow, he thought himself fortunate, but felt so much hurt that he could not attempt to work for a fortnight. He then perceived a swelling, extending all across the lower part of the abdomen, which daily increased in size and painfulness. He sought relief in Loughlinstown Union Hospital, where Dr. Darby, after poulticing for nine days, opened the swelling, evacuating it of a very large quantity of pus. The abscess quickly healed, except a small opening exactly situated opposite, or in front of the external abdominal ring, from which there remained a constant discharge of purulent matter. He returned to his work of ploughing, &c., until the beginning of the present month (April 12). He then was attacked with what he describes as a severe purging of blood, which reduced

him very much, and compelled him again to apply for advice. He sent for Dr. Kemmis, of Kilgobbin, who advised him to come to hospital. He was consequently admitted into the Adelaide Hospital, under my care, upon the 25th of April. Upon examination I found the opening already described over the left external abdominal ring, from which a few drops of very fetid pus exuded; the scrotal tumour was fully as large as a cricket ball, hard, painless, and smooth, but at its lower and back part it felt softer, and there a sense of fluctuation was given to the finger; the integuments of the scrotum were somewhat swollen, and rather oedematous in some parts. While examining the testicle it became apparent that pressure upon it, when applied in certain directions, caused the discharge to flow out of the sinus in the groin. Sometimes a rush of peculiarly fetid pus came out when the sides of the testicle were compressed. The patient stated that when he went to stool, and at other times also, wind escaped freely from the opening. This, he stated, took place before as well as after the discharge from his bowels, which was the immediate cause of his admission—the glands in the groin or elsewhere were not enlarged. Taking the whole group of facts into consideration, the conclusion I arrived at was, that whatever might have been the original nature of the disease of the testicle, that the gland was now the seat of suppuration, and that from it came the fetid discharge, which had caused the internal or pelvic abscess, and which continued to keep open the sinus in the groin. The poor man was very much reduced by the constant discharge, and most anxious to be cured, but he resolutely maintained that his old companion, the big testicle, could have nothing to do with his sufferings—"it had never done him any harm for thirty years." However, when he saw the purulent discharge rush out from the opening in the groin, when the testicle was compressed, he gladly agreed to have the diseased gland removed.

Castration was accordingly performed this morning, and I have now the opportunity of exhibiting an exceedingly interesting pathological specimen. Upon making a section of the mass, we find the testicle perfectly sound at the back of the tumour, the anterior wall of its tunica albuginea being however intimately and inseparably united to the posterior wall of the cavity, which lies in front of it. This cavity is capable of holding a small lemon; it was nearly filled when opened with grumous pus of an intolerable fœtor. When this was washed out it was seen to be lined by a flakey, black substance, beyond which its walls were calcareous, the osseous substance being readily felt, at the cut edge, where it gave the idea of there being bony laminæ in the wall of the cavity. From the very top of this cavity a canal passed up into the cord, capable of admitting an ordinary director. The testicle, although exhibiting healthy gland substance, and in no way involved in the disease,

is seen to be somewhat paler and more atrophied than natural, no doubt from the pressure exerted upon it by the swelling beside it.

What are we to say this suppurating cavity is? Professor Smith suggests to me that the original tumour was an hæmatocele, and this appears to me to be, indeed, the true explanation; for we know that blood in the tunica vaginalis might remain, as this tumour did, perfectly inert and painless for thirty years, and then become inflamed from injury, and suppurate. The course of events in this case seems to be as follows:—1st. An hæmatocele was produced by the kick of a horse, which remained inert and stationary for thirty years—then at last, 2ndly, an abscess is found in the groin, the result of a fall upon a cart-wheel. This communicates along the cord with the hæmatocele. When the abscess is opened, there must have been very little, if any, suppuration in the hæmatocele, for the man returned to his work, suffering no inconvenience for five months, beyond the constant discharge of a small quantity of pus from the opening in the groin. It seems probable that this opening was insufficient for the discharge of the pus forwarded by the suppurating hæmatocele, and that consequently it burrowed downwards into the pelvis, and finally formed the 3rd stage in the case, viz., the formation of an abscess opening into the sigmoid flexure probably, so that when the patient was admitted to hospital he had a suppurating hæmatocele, communicating by a tortuous canal with the opening in the groin, and also with a cavity in the pelvis which opened into the large intestine. From removal of the testicle with its coverings, we may fairly expect a complete recovery.*

The flakey substance lining the cavity we must suppose to be fibrine, while the calcareous plates seem to be the degeneration which blood thus situated sometimes slowly undergoes.

I find in the records of this Society the account of a case detailed by Dr. T. Little in December, 1870, in which a scrotal tumour, which had existed for sixteen years, was carefully examined after death. In describing the contents of the tumour Dr. Little says, "In the midst of a mass of irregular fibres, with a considerable amount of elastic fibre were found numerous oil globules, and hosts of cholesterin plates. In several places small calcareous masses were to be observed." Dr. Little concludes his remarks upon the case by observing:—"Both the history and the result of the examination of the parts seemed to lend probability to the idea that the tumour had originated in an hæmatocele of the testis."

* *May 17, 1872.*—This patient is now nearly quite well. As the wound in the scrotum healed, the sinus in the groin healed also, and is now soundly and perfectly closed.

Vesical Calculi.—MR. PORTER exhibited four specimens of urinary calculi, which he had removed from the bladder within the last six months, by lithrotomy. The first, which weighed two drachms, was a lithic acid stone, and was from a patient seventy-two years of age. He had suffered for six years from irritability of the bladder, and during the last three months his life had become almost intolerable. He was obliged to sit all day in an easy chair, and at night he got very little rest. It was only during the last two months he passed blood in his urine, but latterly, whenever he went out to drive, or take a walk, he suffered from hæmaturia.

The second specimen weighed twenty-five grains, and was also a lithic acid calculus; it was taken from a patient fifty-one years of age. Though the calculus was of considerable size, the patient had only suffered for about two months previous to the operation. The first symptom that attracted his attention was bloody urine, and immediately afterwards the irritation of stone began.

The third specimen was a small lithic acid stone with an oxalate of lime nucleus. The person from whom it was removed was thirty years of age. For nine months symptoms of bladder irritation were present, and every time he mounted his horse (being a cavalry officer) he passed a quantity of blood.

The fourth was a small lithate stone, and was from a patient sixty-eight years of age, who had suffered for nine months. There was an unfortunate complication in this case, for the gentleman had diabetes, but he was quite relieved from suffering so far as the stone was concerned. He had passed blood every day during the month previous to the operation.—*April 27, 1872.*

Intracapsular impacted Fractures of the Neck of the Femur.—Professor R. W. SMITH exhibited two specimens of fracture of the neck of the femur intracapsular, and with mutual impaction of the fragments. In one of them, solid and complete osseous union had taken place; in the other, consolidation was in progress, but nowhere accomplished completely.

The first specimen shown had been preserved for a great number of years (probably forty) in the Pathological Museum of Trinity College. The history of the injury was unknown, the specimen having, most probably, been found in a subject brought in for anatomical purposes. A vertical section of the bone, recently made by Dr. Bennett, rendered it evident that, at some period of the life of the individual, and long before his death, a transverse intracapsular fracture, with impaction, had taken place.

The neck of the bone had lost its natural obliquity, being directed

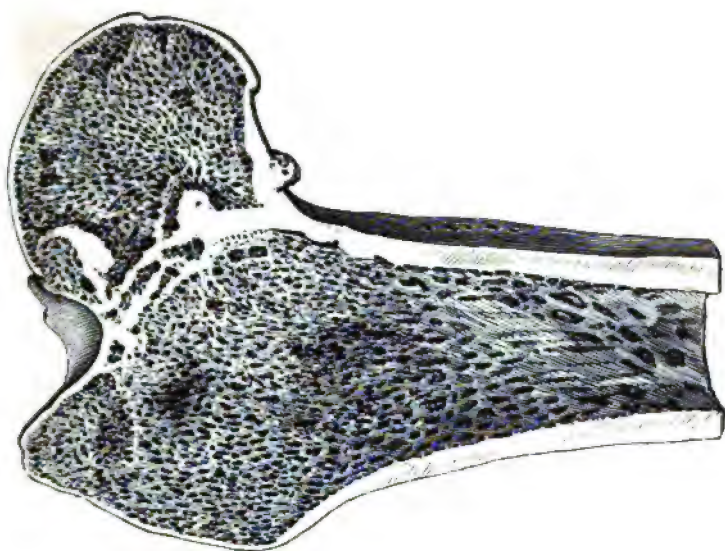
horizontally inwards; the head of the femur was approximated behind and inferiorly to the posterior intertrochanteric line, and to the lesser trochanter, so that the cervix had nearly altogether disappeared, except in front, where a projecting osseous ridge, formed by the anterior margin of the upper extremity of the lower fragment, marked the seat of fracture (Fig. 1). Posteriorly, the line of fracture was close to the corona of the head of the femur.

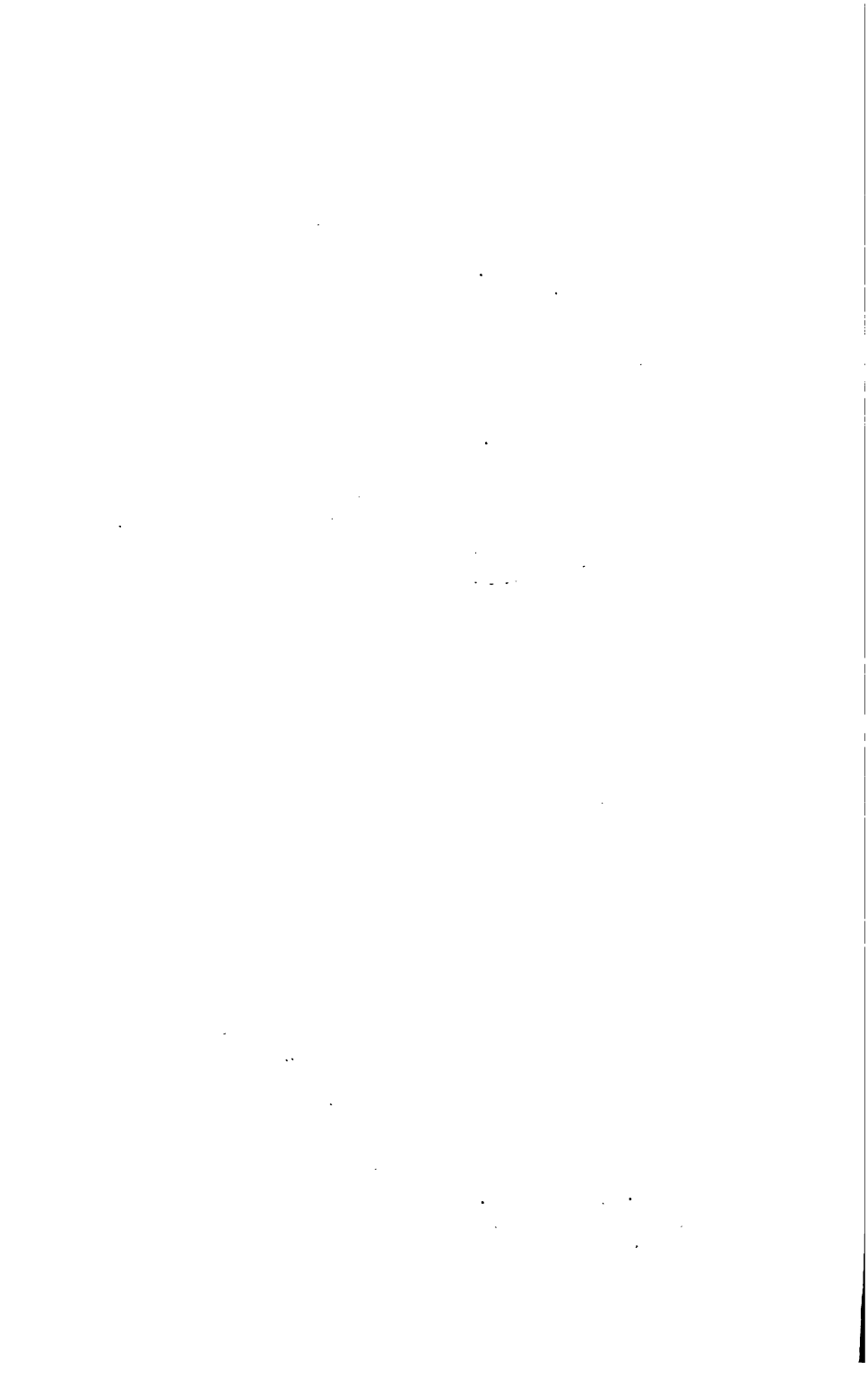
A vertical section of the bone (Fig. 2) disclosed the following appearances:—The head and neck had been mutually impacted into each other, the compact tissue of the latter having penetrated the former, while, both above and below, it was itself overlapped by the reticular structure of the head of the femur. Osseous union, solid, immovable, and perfect throughout the whole extent of the fracture, had taken place between the fragments, the cells of which communicated with each other, except where the compact tissue of the lower fragment intervened.

The second specimen had been recently found by Dr. Bennett in a subject brought to the school for dissection, but the history connected with which was equally unknown. In this case the limb was shortened half an inch and the foot everted. Its inversion could not be accomplished beyond such a degree as permitted of the patella being brought to look directly forwards.

Upon examination, the neck of the bone was found to have been broken above its centre, the line of fracture being everywhere within the capsule. The normal obliquity of the cervix was not as much altered as in the first specimen, but owing, most probably, to the eversion of the limb (in this case greater than usual), a considerable interval existed in front between the fragments; this interval had not become filled up. Posteriorly the corona of the head of the bone was closely approximated to the posterior intertrochanteric ridge.

A vertical section of the bone, carried through the head, neck, and greater trochanter, disclosed a fracture with mutual impaction of the fragments, so extremely similar to that delineated in Fig 2, that I have thought an illustration of it unnecessary. The cellular tissue of the head of the bone had been penetrated both above and below by the compact structure of the cervix, the intervening space being filled up by a projecting portion of the broken surface of the superior fragment. It was here that the process of osseous union had made most progress, but it could not be said to be perfect even here. From the appearance presented in this specimen it is manifest that the injury was of comparatively recent occurrence, and I entertain no doubt but that the case would have terminated in perfect osseous union, had life continued for a few months longer.—*April 27, 1872.*





Renal Tumour.—DR. T. E. LITTLE exhibited an enormous abdominal tumour, with the abdominal and thoracic viscera, which had been removed from the body of a boy, of five years of age, who had died on the previous day in Sir Patrick Dun's Hospital.

The patient had been under observation—almost continuously—from a period of some months antecedent to any perceptible appearance of the tumour. At this early date he suffered from certain urinary ailments of a peculiar character; he was brought to hospital on several occasions, labouring under retention of urine, which came on suddenly from the plugging of the urethra by a soft, tenacious substance, which protruded from the orifice, and was moulded, for a greater or less extent, on these different occasions, to the calibre of that passage. This substance, though frequently examined by the microscope, was always found to be in too decomposed and changed a condition to afford any pathological or diagnostic information.

It was on one of these occasions, when admitted to hospital for the relief of retention resulting from the passage of one of these masses, that a tumour in the right lumbar region first attracted notice. This was in the month of January, 1871, fifteen months before the death of the child. The tumour increased in size, at first very slowly and uniformly, but it evidently became more rapid in its growth, until it finally appeared to fill up the main part of the abdominal cavity, and reached to the size depicted in a photograph of the child, which was exhibited, and which was taken just a month before his death.

At this period, too, a marked local prominence commenced to show itself in the right lumbar region, posteriorly, at the side of the spine. During the last month of life this prominence increased rapidly in size, became red on the surface, and large varicose veins appeared ramifying over it. The child died of exhaustion, after repeated and uncontrollable attacks of diarrhoea.

On making a *post-mortem* examination, the abdominal cavity was found to be almost filled by an enormous tumour, which had thrust aside and encroached upon all the abdominal viscera; the liver was pushed upwards, diminishing the thoracic cavity; the stomach and spleen lay behind, and to the left side of the tumour; the left kidney and the intestines—both small and great, including the ascending colon—were compressed into a small space in the left lumbar and iliac regions. The tumour was extensively adherent to the surrounding parts by bands of adhesion of various ages and strength: in front, it was attached to the walls of the abdomen by slight easily-torn adhesions; in the lumbar region, posteriorly, where the external prominence referred to existed, it was so intimately attached to the skin, that it was impossible (with safety to this structure) to remove the mass without opening into it; the under

surface of the right hepatic lobe was connected to it loosely by bands of adhesion of some length; the cæcum and ascending colon were most intimately blended with the mass at the right and inferior part; the left kidney, small intestines, and bladder, were free. The weight of the abdominal viscera, including the tumour, was 13 lbs. 12 oz.

As to the site of origin of the tumour, both the clinical history and the pathological appearances pointed to the right kidney; no trace of that organ, as a healthy structure, could be discovered; and the right suprarenal capsule—considerably atrophied—occupied a significant position between the tumour and the under side of the right lobe of the liver; the right ureter was pervious, opened normally into the bladder, and, when followed upwards, passed up behind the tumour, which it grooved posteriorly, and in the substance of which it became lost; its superior couple of inches being dilated and filled with a fungoid growth which protruded into it from the mass of the tumour; a large vein passed out from the tumour, which corresponded in position to the left renal vein, and which might be taken to be the emulgent vein of the right side, enlarged to meet the exigencies of the nutrition of the enormous growth.

The tumour was of somewhat spherical or ovoid shape, and possessed a strong and dense capsule in all parts, except where it involved the skin posteriorly. On making various sections of it, it appeared to exhibit a cystic arrangement, having strong and firm septal prolongations of its capsule passing through it, and dividing it into large compartments. The characters of the contents of these compartments differed in different parts; in some, they appeared as numerous, warty, proliferous growths, springing from within, and projecting inwards as large pedunculated masses of a grey colour; this condition was chiefly noticed as occupying a large part of the left half of the tumour, and also in that part of it which tended to present externally in the lumbar region; in others, the spaces were filled with a soft, elastic, cerebriform description of matter, which formed the main bulk of the tumour; while, in others, the contents of these compartments were composed of soft caseous masses, easily broken down, and in places quite different.

Throughout both lungs there existed scattered masses—of the average size of a small pea—of the same appearance as the cerebriform matter found in the tumour. The glands both of the abdomen and thorax appeared to be free from disease.

Microscopic examination showed that the mass, generally, reproduced the characteristic appearances of a round-celled sarcoma, with considerable differences in its different parts. Corresponding to the rough classification given of a threefold variety of naked-eye appearances just detailed, the warty growths alluded to, while exhibiting a characteristic cell element in groups of small nucleated, more or less spherical cells,

possessed an unusual amount of intercellular substance, with ramifications of large caudate, connective-tissue corpuscles, but which did not yield any mucin to chemical tests. The more solid parts of the tumour presented very typically the ordinary appearances of the round-celled sarcoma, with a considerable degree of fatty and colloid degeneration of the cells; while in the caseous parts of it the process of fatty degeneration seemed to have reached to the highest degree, the microscope detecting cells, advanced in fatty degeneration, fatty, and, in places, masses of mineral granules, and an abundance of the granular corpuscles of Gluge.

The microscopic appearances of the secondary depositions in the lung were identical with those presented by the most typically sarcomatous parts of the tumour, with, however, a lesser amount of degenerative process of the cells, as indicative of their more recent formation.—*April 27, 1872.*

Cardiac Dropsy dependent on extreme and rapid Dilatation of the Heart : Hydropericardium.—Dr. W. G. SMITH said: On Monday, March 4, a young man, aged twenty-two, was brought into the Adelaide Hospital, labouring under anasarca with marked lividity; and on the following Sunday he died.

The circumstances of the case, and the rapidly fatal termination of it, without any physical signs of valvular or fatty disease of the heart, invested it with some interest, especially as considerable obscurity and difficulty attended the diagnosis during life.

Though in the habit of drinking a good deal without incapacitating himself, however, from his occupation as an electrical instrument maker, this man had enjoyed perfect health, with the exception of an attack of rheumatism in his legs some months ago. He remained in good health till six weeks since, when he contracted a cold in the chest, and was under medical treatment for this affection. One month ago œdema of the feet set in, which gradually extended up to the abdomen; the cough continued troublesome, attended with dyspnœa, and thin, mucous expectoration, but no hæmoptysis or hæmorrhage from any situation. Even while dropsical he continued to work till within a few days of his admission to hospital; and when in hospital he made light of his case, although the dropsy of the legs continued. There was marked congestion all over the body, especially in the lips, ears, hands, and feet, and yellowness of the sclerotics.

He had no extreme dyspnœa, and was able to lie with comparative ease on his back; there was no enlargement of the abdominal veins; no pulmonary physical signs, except a moderate degree of dulness below the scapulæ. The legs pitted readily and deeply, and the abdomen measured

38 inches. The impulse of the heart was widely diffused and undulatory, extending from below the nipple to the second rib, and the percussion-area was increased transversely. The cardiac action was irregular, but no murmur was audible at any time. He never complained of palpitation or of præcordial pain. No jugular pulsation, no undue fulness; the radial pulse was almost imperceptible, and averaged 80.

The urine was scanty and not albuminous. He seemed to hold his ground for a few days, and even to improve slightly. But after a little time he became drowsy in the daytime, and on Saturday felt there was something wrong with his tongue. Next day, Sunday, he wore an anxious expression. The orthopnœa was more decided. A curious symptom now presented itself. On looking into the mouth, the tongue was seen to be very much swollen on the left side, being nearly an inch in thickness, brownish, tense and elastic to the feel, and scarcely prehensible from the mouth; the cedema extended to the sublingual region, but there was no appearance of true glossitis. He got up about noon, and at three o'clock in the afternoon he asked to be lifted back into bed, when he fell into the attendant's arms, weak and unable to move, and complaining of want of breath. The resident pupil saw him, but he died in five minutes, without any kind of struggle, and retained his consciousness almost to the last moment.

On opening the chest the pericardium was seen to bulge forward, and measured $6\frac{1}{2}$ by 6 inches, and yielded half a pint of yellowish limpid serous fluid, but there was no trace of pericarditis. The heart was widely dilated, and measured 6 inches by $4\frac{1}{2}$; the right auricle 4 inches by 2, and both it and the right ventricle were full of dark, grumous, soft clots, but there was no thrombus. Every valve was healthy and perfectly competent, except the mitral, which was, perhaps, slightly dilated. There was no excess of fat on the heart, and the muscular tissue was found to be healthy. There was no effusion into either pleura, but both lungs were universally and almost uniformly congested.—*April 27, 1872.*

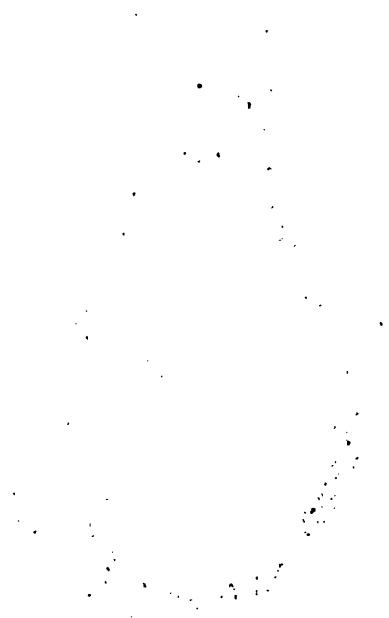




Fig. 1.



Fig. 2.



Fig. 3.

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MEDICAL SCIENCE.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. III.—*Surgical Reports.* By GEORGE H. PORTER, M.D., Dub., F.R.C.S.I.; Surgeon in Ordinary to Her Majesty in Ireland; Senior Surgeon to the Meath Hospital and County of Dublin Infirmary; Ex-President of the Royal College of Surgeons in Ireland.

- I.—TIGHT ORGANIC STRICTURE OF THE MALE URETHRA; A LARGE PORTION OF A CATHETER LODGED IN THE BLADDER; THE STRICTURE FORCIBLY DILATED; THE FOREIGN BODY EXTRACTED; RECOVERY.
- II.—A PORTION OF A GUTTA-PERCHA BOUGIE BROKEN OFF IN THE BLADDER, CALCULOUS FORMATION AROUND IT; A MODIFICATION OF "ALLARTON'S LITHOTOMY;" RECOVERY.
- III.—NINE INCHES AND A HALF IN LENGTH OF A GUM ELASTIC CATHETER LODGED IN THE MALE BLADDER, SURROUNDED BY PHOSPHATIC DEPOSIT; A MODIFICATION OF "ALLARTON'S LITHOTOMY;" RECOVERY.
- IV.—PATIENT AGED TWENTY-SIX YEARS; A SMALL LITHATE CALCULUS, WITH AN OXALATE OF LIME NUCLEUS; LITHOTRITY; RECOVERY.
- V.—PATIENT AGED FIFTY-ONE YEARS; SMALL LITHATE CALCULUS; LITHOTRITY; RECOVERY.

ALTHOUGH substances of the most varied forms and composition have been found in the female bladder, demanding surgical interference for their removal, few foreign bodies are met with in the male

viscus, with the exception of portions of bougies, or catheters. The extraction of these from the female is an easy procedure compared with their withdrawal from the male patient. The explanation of this is simple, when we consider the anatomy of the urethra in women, its great dilatibility, and its short and slightly curved course. The length of time a foreign substance has remained in the bladder influences, in a great measure, the surgeon in bringing his practical knowledge to the aid of the sufferers. The knife, or lithotrite, is usually required when the substance has continued long enough to become the nucleus of phosphatic deposit. When called upon to relieve a male patient suffering from a foreign body in the urinary bladder, the practical surgeon must earnestly consider all the features of the case before rashly proceeding to any operation. He should endeavour to ascertain the nature of the substance, whether tough or brittle, its shape, the length of time it has lodged in its unnatural situation, and, lastly, the condition of the bladder as affected by its presence. No doubt, in some cases where the urethra is large, and the foreign body small, it may at once be seized by a lithotrite and taken out, or if friable it may be broken up, and allowed to be washed away with the urine, like a calculus. This is on the supposition that the state of the bladder is healthy. But, on the other hand, if it be tough, and incapable of being acted upon by the lithotrite, and that it is lying in a viscus inflamed and irritable, then the surgeon should not delay to remove it by the knife. For the removal of foreign bodies which cannot be extracted along the urethra, lithotomy through the middle line of the perinæum is, *par excellence*, the operation to be chosen. The highest authorities agree on this point. Mr. Erichsen says—"This is more safely done by the median than by the lateral operation of cystotomy." Ashurst states, when treating of foreign bodies in the bladder—"In the male, however, it is usually necessary to resort to lithotrity (if the nature of the body admit of its being crushed), or to lithotomy, the *median* being in such a case the preferable operation." Again, Mr. Bryant writes—"In adults, possibly, the median operation may be chosen when the foreign body is not large."

There is a difference of opinion among surgeons as to the best way of reaching the staff, and by it the bladder, in central lithotomy. Allarton plunges his knife at once, with its back to the rectum, into the membranous portion of the urethra, placing the point of the bistoury half an inch in front of the anus. In the old "*marian*"

operation a cut was made on the right or left of the raphe upon the staff, beginning below the scrotum, and terminating an inch above the anus. Sir Henry Thompson says—"Respecting the modes of making the incisions, I prefer dissecting from the skin inwards to the staff, as in other operations, to the method by transfixion." He does not mention where he begins his incision through the skin.

Mr. Erichsen recommends a rectangular staff, resembling Buchanan's, to be used, and considers that "the surgeon can judge of the exact point where to enter the knife (directing it so as to open the groove just below the angle) which he cannot do with the curved staff."

I consider the recommendation of Sir H. Thompson to cut from the skin inwards most useful, particularly when the perinæum is deep. In such a case the whole blade of the knife would be swallowed up if driven to the staff, according to Mr. Allarton's advice, and the heel of the blade would be brought to bear on the tough skin of the raphe; whereas the free division of the integument, in the first instance, facilitates the further separation of the structures to be cut through.

The operation I have four times successfully performed, and which I take the liberty of suggesting to others, is a combination of Allarton's and Thompson's; but differing slightly from both. It is briefly described as follows:—The staff, grooved on its convexity (and as large as possible) having been introduced, kneeling in front of the patient, I insert the forefinger of my left hand into the rectum with its palmar surface upwards, and fix it against the staff at the apex of the prostate gland. I then push Allarton's bistoury about a quarter of an inch in depth with its back to the bowel, half an inch anterior to the anus in the raphe, along which I then cut one inch and a half in length towards the root of the scrotum, freely dividing the skin, and superficial fascia. This having been done, I return the point of the knife to its first starting place, and quickly plunge it to the staff at the apex of the prostate, making sure that I have freely opened the urethra, by moving the bistoury in the groove of the staff. I then cut forwards to the extent of the superficial incision. Still keeping the staff steadied with the point of the finger in the bowel, I introduce a long steel probe along the groove of the instrument into the bladder. Then having withdrawn the finger from the rectum, I introduce it on the probe, and dilate the prostate by insinuating the finger with a rotatory motion.

This being accomplished, the forceps is passed in on the finger, the foreign body caught, and at once extracted. The following cases of foreign bodies in the bladder, and the surgical exertions for their removal, may be interesting to the profession:—

CASE I.—Tight Organic Stricture of the Male Urethra; a large Portion of a Catheter lodged in the Bladder; the Stricture forcibly dilated; the Foreign Body extracted; Recovery.

T. C., aged forty-three years, a married man, had suffered for three years from tight organic stricture of his urethra about four inches from its orifice. It was caused, apparently, by an injury received nine years before. He had it dilated on several occasions by different modes of treatment, but it always showed a great tendency to close again. He had been three times attacked with complete retention of urine, and was with great difficulty relieved. He was in the habit of introducing bougies himself, and allowing them to remain in the passage for some hours, and for the last two years felt satisfied with his own treatment without applying to a surgeon. He was never able to force a bougie larger than No. 4 through, or rather into the stricture.

On the 28th June, 1872, he came under my observation, and gave the following details:—He had cut off about five inches and a-half of a French bulbous catheter, which he forcibly pushed into the stricture, where he left it so tightly grasped, that he thought it impossible for the instrument to come out, or, on the other hand, to glide backwards into his bladder. He went out for a drive of two hours' duration, and on returning home discovered to his great alarm that, instead of remaining as he had placed it, the piece of catheter had worked its way through the stricture, and lay partially in his urethra and bladder. I thus found him in a state of great anxiety, complaining of acute pain in the region of the bladder, with a constant desire to pass water, which dribbled away on each exertion to avoid it. Pressure on his perinæum gave intense agony. Having endeavoured to calm his mind, I had him placed immediately in a warm bath, and administered a draught containing tincture of hyoscyamus, liquor potassæ, laurel water, and camphor mixture. After remaining in the bath for twenty minutes I had him removed to bed, and told him I would endeavour to extract the piece of the catheter early the following day. He was desired to repeat the warm bath in the meantime, as also the draught at ten o'clock p.m.

Here was obviously a most anxious case, and one that called for the best consideration prior to the adoption of operative measures. The stricture forbade the introduction of a forceps or lithotrite, to seize the foreign body, and the same difficulty was a bar to my passing a fair-sized staff, on which I might cut into the bladder. After having carefully weighed all the difficulties, I determined in the first instance to dilate the stricture, and then with a lithotrite catch and extract the piece of the catheter; but should I fail in this, at once to cut into his bladder and take it out.

June 29th.—Assisted by my friends Mr. Wharton and Dr. M'Cormick, I had the patient placed fully under the influence of chloroform; I then introduced "Holt's Dilator," and burst the stricture; on the withdrawal of the dilator only three or four drops of blood followed. I then passed a No. 9 silver catheter, to be certain that a way sufficiently large was made for a lithotrite, and also to push on the foreign body fairly into the bladder, should it be in any degree lodged in the prostatic portion of the canal. The stopper was allowed to remain in the catheter, to prevent the urine escaping, and as I passed the metallic instrument I distinctly felt the offending substance. I then rapidly withdrew the catheter, and replaced it by a small-sized lithotrite, which was with the greatest facility introduced. With this I felt the piece of the catheter lying to the left side of the patient's bladder, and having opened the blades of the lithotrite, I was fortunate enough to grasp it and extract it slowly, but without difficulty. I then drew off his urine, introduced a quarter grain morphia suppository into the rectum, and placed him comfortably in bed, with warmth to his feet.

June 30th.—He had a quiet night, and was free from vesical irritation. From this date he made a rapid recovery, and was about his business the sixth day after the operation. Probably no case could be presented more suitable to the "immediate plan" of treating stricture, and certainly the scientific course to adopt was the one which I pursued previous to resorting to the extraction of the foreign body by a cutting operation. The method I adopted was crowned with the most gratifying success. Plate IX., Fig. 2, gives a good representation of the piece of the catheter removed.

CASE II.—A Portion of a Gutta-Percha Bougie broken off in the Bladder; Calculous Formation around it; a Modification of "Allarton's Lithotomy;" Recovery.

J. M., aged twenty-five years, was admitted into the Meath Hospital under my care, May 8th, 1870. He had suffered from organic stricture for more than twelve months, and had been in the habit of treating himself by introducing bougies. About seven months previous to admission he had constructed a bougie from a piece of gutta percha, and on the first occasion that he passed it into his bladder a portion about three-quarters of an inch in length broke off when he was withdrawing the instrument, and this subsequently became the nucleus of a stone. When he came under my observation he was suffering from all the symptoms of a calculus in the bladder, and his health was greatly broken down. He got very little rest, his urine was loaded with pus, and contained blood. A No. 5 sound was the largest that the stricture would admit, and with this the foreign body was easily found. The contracted state of the urethra, and the highly inflamed condition of the bladder in a man so debilitated, prohibited the idea of removing the stone and foreign body with a lithotrite. I determined to cut it out.

Operation.—May 13th.—His rectum having been cleared out by a tepid water enema, at 10 o'clock a.m. he was brought into the operation theatre, and put fully under the influence of chloroform. He was quickly tied, and held in the usual position for lithotomy. A No. 5 staff grooved on its convexity was introduced, and firmly held by my colleague, Mr. Wharton. Then, kneeling, I inserted the forefinger of my left hand into his rectum with its palmar aspect upwards, and its point steadily pressing on the staff at the apex of the prostate gland. I then pushed Mr. Allarton's knife with its back towards the bowel a quarter of an inch deep, striking the perinæum half an inch in front of the anus. I then cut forwards along the raphe to the extent of one inch and a half, dividing the skin and superficial fascia. I next fixed the point of the bistoury where I commenced my incision, and plunged it (with the cutting edge still away from the rectum) deeply until I lodged its point fairly in the groove of the staff at the membranous portion of the urethra. Having made certain that I had safely entered it, I cut forwards to the extent of the superficial incision. I then laid aside my knife, but still keeping my left forefinger in the bowel,

I passed in through the wound a long steel probe which I caused to glide along the groove of the staff into the bladder, proving that I was in the proper course by striking the end of the probe sharply against the stop at the extremity of the staff. And here let me recommend in the strongest manner the advantage of using a staff constructed with this abrupt resisting point in every mode of lithotomy, whether the surgeon hits it with the point of his knife in the lateral operation, or with the end of the probe as in the procedure under consideration; it proclaims that he has travelled the direct route, and is safely in the bladder. I then had the staff removed, and at the same time withdrew my finger from the rectum. Still firmly holding the probe with my right hand, I inserted my left forefinger (which had served whilst in the bowel to direct the knife in safety to the staff) along the probe into the bladder, and by rotating it two or three times I largely dilated the prostate. I felt the foreign body lying at the bottom of the viscus, below the level of my finger. I then took out the probe, and on my finger insinuated a small lithotomy forceps with which I quickly seized the stone, and brought out a large portion; it broke, however, under the grasp of the forceps, so that I was obliged to remove the remainder of it by a second introduction of the instrument. I was aided by a careful washing out of the bladder by means of a strong syringe. The patient was now placed in a well-warmed bed, and had a morphine suppository passed into his rectum, and directions were given to keep him as dry as possible by placing folded sheets under his buttocks as often as required. The amount of bleeding was remarkably trifling, and the wound needed no dressing.

May 14th.—He had a most refreshing night, was free from the constant calls to micturate which disturbed him before the operation; his urine flowed freely through the wound, and his pulse counted only eighty; he did not suffer from any febrile symptom.

May 16th.—He expressed himself as greatly relieved, all feeling of bladder irritation had subsided, and the urine passed constantly through the wound. It would be tedious to relate the daily condition of the patient; he improved in health rapidly, the urine began to flow through the urethra on the sixth day after the operation, the wound closed in the most healthy and satisfactory manner, and he left the hospital cured four weeks after the removal of the foreign body.

A very good sketch of the piece of the bougie surrounded by the calculous formation is given in the lithograph, Plate IX., Fig. 3, by Forster & Co., drawn by his accomplished artist, Mr. Tomsohn.

CASE III.—Nine inches and a half in length of a Gum Elastic Catheter lodged in the Male Bladder and surrounded by Phosphatic Deposit; a Modification of Allarton's Lithotomy; Recovery.

J. T., aged thirty-eight years, by trade a plumber, was admitted into the Meath Hospital, under my care, November 28th, 1872. He had suffered from stricture of the urethra for two years previous to his admission into hospital, and had himself been in the habit of passing bougies and catheters occasionally during the last eighteen months of that period. Nine weeks prior to coming under my notice, in withdrawing a gum elastic catheter (No. 6), which he had introduced, it broke across, leaving one inch and a half of the instrument between his fingers and thumb. The larger portion he immediately tried to take out of his urethra, but failed in the attempt, and, in about an hour afterwards, it slipped into his bladder. For two days subsequently he felt no annoyance, and was able to follow his usual occupation. On the third day, however, after the accident, he felt great pain during, and at the termination of, micturition in his glans penis; his urine was bloody, and any sudden motion increased his suffering. He placed himself under the care of a surgeon who commenced to dilate the stricture before attempting the removal of the foreign body; but the patient became anxious for speedy relief, and left the hospital while he was undergoing treatment. He then applied to my friend Dr. Trimble, of Castlebellingham, who detected the foreign substance in his bladder, and recommended him to come to me.

On admission to the hospital, I found him suffering from frequent calls to make water, so often as ten times during the night. His urine was bloody, and contained pus and ropy mucus. One small piece of the exterior of the catheter had been expelled, and numerous fine fragments of phosphatic deposit also came away.

November 29th.—I sounded him with Sir Henry Thompson's sound, and soon felt the catheter which, on being struck, gave the sensation as if it were more or less encrusted with calculus matter. I ordered him to remain in bed, and prescribed sedative medicine, with diluent drinks. In consultation with my colleagues, it was considered a case not suitable for an attempt to extract the foreign body by the urethra. The catheter, it was considered, might break (if caught in a lithrotrite) into pieces, which could not be washed away with the flow of urine, but remain to form the foundation of other deposits. And even if it did not

thus become disintegrated, it could not be drawn through the urethra in its coated condition without lacerating the canal to a dangerous extent. The highly inflamed state of his bladder also prohibited such instrumental interference. I therefore decided upon performing the operation which I have above described, and which I strongly recommend. I operated on the morning of December 4th, and removed the portion of the catheter, a faithful delineation of which from the pencil of Mr. Tomsohn, and lithographed by Forster & Co., is to be seen, Plate IX., Fig. 1.

The operation was done under the influence of æther, which was kindly administered by my colleague Mr. Smyly, in a very efficient manner.

The patient made a very satisfactory recovery. The urine passed through the penis on the fifth day, and he left the hospital, with the wound healed and in fine health, on the 7th January, 1873.

Remarks.—The accident of a catheter or bougie slipping backward into the bladder is by no means so uncommon as is generally supposed; and every surgeon should carefully examine instruments prior to using them. Age renders them brittle and unfit; and, even when their condition is sound, the utmost precaution should be adopted (when allowing them to remain in the urethra) to secure them in such a manner that they cannot recede, or glide forwards from the canal. When a portion of a soft bougie or catheter is for a short period lodged in the bladder, it is not an easy procedure to feel it with a sound; and this fact is alluded to by Mr. Fleming, a gentleman who has devoted much attention to injuries and diseases of the urinary organs, in his description of a case in which a bougie—which had been introduced and left in the urethra—escaped into the bladder.—*Dub. Hosp. Gazette*, Sept. 1, 1858. A very interesting case is reported by my friend, Mr. William Stokes, Professor of Surgery in the Royal College of Surgeons, in the first number of the *Irish Hospital Gazette*. This was a case of the removal of an *entire* gum elastic catheter from the bladder by Allarton's operation.

CASE IV.—*Patient aged Twenty-six Years; A small Lithate Calculus, with an Oxalate of Lime Nucleus; Lithotritry; Recovery.*

H. B., a cavalry officer, aged twenty-six years, consulted me November 15th, 1871. He had suffered for fourteen months previously from great pain after micturition, and irritability of the

bladder. Whenever he rode on horseback his urine became bloody, and even when blood was not apparent to the unaided eye the fluid had a smoky hue, and was turbid. Prior to this date he had enjoyed excellent health, and was able to take the most active exercise, in addition to the ordinary duties of his regiment. He was now obliged, however, to go continually on the sick list, feeling himself disabled for duty, and his sufferings were so great that he contemplated throwing up his profession. He had been under the care of three different surgeons, who from time to time prescribed medicines calculated to alleviate the irritable condition of his bladder, and he had been sounded by each of them without detecting the presence of a stone. He had a capacious urethra, and bore very well the introduction of an instrument. I passed Sir Henry Thompson's sound into his bladder with ease, and after turning its beak from side to side without striking the calculus, I at last hit it on turning the point of the sound down behind the prostate close to the neck of his bladder. The click which was given led to the supposition that its formation was hard, but that its size was not considerable. In this examination I was ably assisted by Surgeon-Major Gilborne, who had given his opinion that the gentleman suffered from stone. And here I may say a word respecting Sir H. Thompson's sound, which I consider every surgeon who treats urinary diseases should have in his possession. I believe many stones have been discovered by its use which would have escaped the search made with the ordinary sounds constructed with large curves. A calculus of small size may lie undiscovered beneath the long curve of an instrument, while the short bend of Thompson's sound can hardly fail to find it when turned completely round in the bladder. I should add that the cylinder attached to the handle not only gives a most convenient hold of the instrument, but intensifies the noise if it strikes the foreign body, even in the most gentle manner. I told the patient to remain in the recumbent position as much as possible, and I prescribed a mixture containing tincture of hyoscyamus, four drachms; laurel water, two drachms; camphor mixture eight ounces—to take an ounce every third hour; whilst I desired that he should drink every day a pint of decoction of triticum repens.

November 18th.—*First Sitting.*—Having placed him on a low bed, with a hard hair mattress under him, two assistants held his legs flexed in the manner usually fixed for lithotomy. I introduced Sir Henry Thompson's flat-bladed lithotrite, and soon caught the

stone, which measured half an inch in the diameter which was seized. I crushed it once, and withdrew the instrument. He bore the operation well. I then put a quarter grain morphine suppository into his rectum, and gave him a glass of champagne. He was instructed to remain on his back in bed, and to make water in that posture; to have plenty of oatmeal tea as a drink, and chicken broth during the day. I visited him on the evening of same day, and was told that he felt relief, and was less frequently obliged to micturate. The urine was not bloody, and no detritus had come away. I then ordered him a draught containing twenty grains of hydrate of chloral.

November 19th.—He had passed a restless night, not from pain or vesical irritation, but he felt nervous and wakeful, and a small quantity of *debris* had been expelled, weighing five grains. He was ordered to remain quiet in bed, and to partake of the same diet as the day before, the chloral draught to be repeated at bedtime.

November 20th.—He had a good night, and felt greatly refreshed. This morning I washed out his bladder with Clover's apparatus, but with little effect, the amount of detritus being almost *nil*.

November 22nd.—*Second Sitting*.—I used the same instrument as at the first crushing, and caught two fragments, each measuring about a quarter of an inch in diameter, and I broke them with ease. The lithotrite on this occasion brought out between its blades a large quantity of pulverized stone. On visiting the evening of this day I found that a large amount of debris, weighing twenty-two grains, had passed out. The greater portion came away with a rush on his making water, about two hours after the operation. The chloral draught was taken again this night.

November 26th.—He felt very much relieved, having slept soundly, but no fragments passed away.

November 27th.—I explored his bladder with Thompson's sound, and was unable to feel any piece of calculus. I then permitted him to sit up in an arm-chair, and to have some chicken for dinner, with a glass of champagne.

November 28th.—He had a good night, and no detritus came away.

November 30th.—He was so well, and free from all irritation, I allowed him to go to Bray for change of air, where he remained for ten days.

December 11th.—He returned to town greatly improved in

general health, but his urine was not clear, and he felt a sharp pain at times whilst expelling the last drops of water. I then sounded him again under the influence of chloroform, assisted by my friends Mr. Wharton and Surgeon-Major Gilborne. On this occasion I used a flat-bladed lithotrite in sounding, as I could with ease, and at once, crush any fragment, and as I should escape, by its means, the trouble and consequent irritation of introducing the second instrument. I was unable to find any piece of calculus.

December 19th.—*Third Sitting.*—Up to this date he progressed very well, was able to drive out every day without pain, and retained his water for four hours and a-half. There was no hæmaturia, but the urine continued rather opaque, and he noticed occasionally a sudden check in the stream. I made him empty his bladder, and then introduced a small flat-bladed lithotrite, and almost instantly caught a small fragment, and reduced it to powder. This piece measured one-eighth of an inch, as indicated on the scale of the lithotrite, and was brought out in the jaws of the instrument. From this date the urine became perfectly transparent, all symptoms of irritation ceased, and my patient made a rapid recovery. In a letter dated January 4th, 1871, he says, "I feel now perfectly well, and rode out with my regiment for two hours this morning." The weight of all the detritus preserved was twenty-eight grains. A small triangular nucleus of oxalate of lime was found in the debris passed on the 22nd of November.

CASE V.—Patient aged Fifty-one Years; Small Lithate Calculus; Lithotrixy; Recovery.

W. S., aged fifty-one years, had suffered from frequency of passing water for three months previous to coming under my observation. The first symptom of irritation he perceived was immediately after a sudden exertion in pursuing a run-away horse. He then expelled bloody urine, and felt great uneasiness in his bladder, with pain at the extremity of his penis. He was healthy-looking, and passionately fond of horse exercise, but latterly was unable to ride for the space of half-an-hour without dismounting to micturate. His urine had a smoky tinge when the red colour subsided.

November 7th, 1871.—Assisted by Mr. Wharton, I sounded him, and detected a calculus, which from the click imparted by striking Sir Henry Thompson's sound against it led us to infer that the stone was tolerably hard, but of small size.

November 9th.—*First sitting.*—I placed him in the position for lithotrixy, on his bed, with his buttocks raised by means of a pillow. He had been directed to retain his water for two hours. I then slowly introduced Thompson's flat-bladed lithotrite, slightly heated and well oiled, and soon felt the stone lying at the right side of his bladder. I immediately separated the blades of the instrument, easily caught the calculus, and crushed it. I found it so difficult to screw home the male blade that it was evident a large quantity of detritus lay between the blades, and, without attempting to break a second piece that day, I withdrew the instrument. In so doing a good deal of pain was produced by its increased size, particularly at the orifice, which I was obliged to slightly lacerate in extracting the beak of the lithotrite. I placed a quarter grain morphine suppository in his rectum, ordered him a glass of champagne, and to have oatmeal tea to drink *ad libitum*, with chicken jelly every third hour.

November 10th.—He passed a good night; no fever; several small fragments came away with the urine, which he expelled in the recumbent position.

November 11th.—Very little debris found, but he had a tranquil night.

November 12th.—He had more frequent calls to make water, and a small amount of detritus got rid of. Ordered to have a chloral draught at night.

November 13th.—He slept well, and felt most refreshed. A good deal of debris, chiefly in fine particles, passed off. From this date to the 17th he was able to sit up, and had but slight inconvenience; his urine, however, was opaque, and he felt occasionally at the end of micturition a sharp sting.

November 17th.—*Second sitting.*—I passed a small-sized flat-bladed lithotrite, and seized a fragment measuring a quarter of an inch. I reduced it to powder, and brought it out between the blades.

November 25th.—I washed out his bladder with Clover's instrument; but the result was but a trifling amount of debris.

November 27th.—*Third sitting.*—I passed in the flat-bladed lithotrite, and after searching for a minute or two, I seized a fragment of stone by turning the curve of the instrument downwards. It measured one-eighth of an inch. I screwed home the lithotrite, and brought out the disintegrated particles in the female blade.

November 29th.—The patient felt very much improv-

health. Since the last crushing he had passed a considerable quantity of detritus, along with two large fragments, one weighing four grains, the second two grains and a half.

December 1st.—He was allowed to go out for a short walk. On his return, in less than an hour, he felt great urgency to pass water, and whilst so doing two large pieces of calculus were expelled with force. It should have been noticed that up to this date his urine never became perfectly limpid.

December 9th.—*The fourth and last sitting.*—I introduced the flat-bladed lithotrite again, and grasped a fragment which the index of the lithotrite marked to be less than one quarter of an inch in the diameter caught. It was immediately reduced to fine detritus, and brought out between the blades of the instrument.

After this operation his urine daily became transparent. He lost all vesical irritation, and left Dublin quite well.

On the 8th January, 1872, I received a letter from him in which he states—"I never was better in all my life, no stony symptoms of any sort remaining, and the water still continues perfectly clear. I have given it a fair test, having been constantly hunting since." The aggregate weight was thirty-six grains. A remarkable feature in this case was that although the stone had increased to a considerable size, still the patient had only been suffering from irritation of his bladder for two months previous to its detection.

ART. IV.—*Contributions to Operative Surgery.* By SAMUEL T. KNAGGS, M.B. Ch.M. (Aberd.), F.R.C.S.I., L.K. & Q.C.P.I.; Honorary Surgeon to Newcastle Hospital, New South Wales; Assistant Surgeon N. S. W. Naval Forces.

CASE I.—*Ovariectomy; Recovery; Observations.*

MISS M. W., aged twenty-nine. The early history of this case is somewhat obscure; no definite information could be obtained concerning the symptoms due to the incipient stage of the disease. For years the patient had suffered from ill health and general delicacy of constitution; one of the most prominent symptoms was an almost continual pain in the left side. She had also been several times under medical treatment for membranous dysmenorrhœa. On two occasions there was a history of stoppage of the menses and consequent enlargement of the abdomen; excepting these periods she had always been most punctual in menstruating.

About January, 1871, she accidentally got a severe fall, which she believed caused some internal injury. On Good Friday following (April 7th), she first noticed herself getting larger by observing her corset fitting very tightly; then she noticed a greater prominence on the left side of her abdomen. From that date a gradual and almost imperceptible enlargement took place. Her menstruation appeared for the last time on February 2nd, this year, a period of three weeks having elapsed since the previous discharge. On 28th March, I was consulted for the amenorrhœa and enlarged abdomen, which, since the stoppage of the menses, rapidly became more prominent. The patient being unmarried, and the case not then being sufficiently urgent to justify my pressing a vaginal examination, I put her upon tentative treatment, and directed my attention to the improvement of her general health and restoration of the menses by the usual measures.

Two successive periods passed under this treatment without any satisfactory result. The girth of the abdomen at the umbilicus increased from 31 to 40 inches, and the symptoms became more urgent. More accurate diagnosis was now rendered imperative; the patient was carefully examined. No defined tumour could be detected in the abdomen, which was tense and elastic, affording an obscure sense of fluctuation, and not altering its shape on the patient changing posture. There was dulness to percussion on the anterior aspect of the enlargement, slight resonance at the sides, and high up beneath the diaphragm. The uterus was healthy, of a normal size, but the axis displaced, looking downwards and slightly forwards, the whole organ being pushed bodily backwards, almost into the hollow of the sacrum. The history of the case and the result of this examination clearly pointed out ovarian disease. To confirm the diagnosis, as well as to relieve pressing symptoms, an exploratory puncture was proposed and acceded to by the patient. Accordingly, in May, after the agonies of frustrated menstruation had ceased, I tapped her through the linea alba, midway between the umbilicus and pubes. At first a few drops of ascitic fluid oozed out of the cannula, the internal orifice of which was obstructed by a hard elastic body; the trocar was again introduced, the elastic body punctured, and then out gushed a quantity of thick yellowish brown fluid of a syrupy consistence, the exact quantity being four and a half pints. Before withdrawing the cannula it was used after the manner of a sound, and by that means I found that other hard elastic bodies were behind and surrounding the one that was

punctured. The size of the abdomen being considerably decreased by the tapping, it was found by palpation and palpation that an uneven, irregular, elastic tumour occupied the cavity of the abdomen. The case was diagnosed to be a multilocular ovarian tumour.

The facts of the case were plainly placed before the patient and her friends, to whom the extirpation of the tumour was suggested, and the terrible risk of the operation fully explained. The onus of decision being thrown upon me, I advised operation as the only means by which a permanent cure could be obtained, but recommended that it should not be attempted until the actual recurrence of such symptoms as should absolutely require interference in order to afford relief. We had not long to wait; in a fortnight the patient was fully as large as before the tumour was tapped; the following week menstrual disturbance without menstruation caused the most acute suffering, and so rapidly did the tumour grow that the girth of the abdomen at the umbilicus was $43\frac{1}{2}$ inches; respiration became impeded, the natural functions of the body were threatened with interruption, and relief was imperatively demanded.

On the 24th June the patient was moved into the Newcastle Hospital, in order to undergo the operation, which was performed at noon on the 26th. The night before an aperient draught was administered, and on the morning of the operation the bowels were cleared out with a soap and warm water enema, and an early breakfast given. Doctors Harris, Schroeder, and Robert Pierce were kind enough to be present and assist at the operation. The patient having been fully placed under the influence of chloroform, an incision two and a half inches long was made in the abdominal parietes, through and in a direction with the linea alba, midway between the umbilicus and the pubes. The peritoneum being reached, it was pinched up in a forceps, divided on a director, and the tumour exposed. A cannula ($\frac{1}{4}$ inch in diameter, attached to an india-rubber tube) was plunged into the presenting cyst; the contained fluid—thick, viscid, yellow in colour, and of the consistence of the albumen of an unboiled egg—gushed out at the sides of the tube, but very little through it. The tube proving comparatively useless, it was removed, and the orifice in the cyst wall drawn out beyond the abdominal incision by means of torsion forceps. Pressure being kept on the sides of the abdomen, the tumour completely plugged up the abdominal incision. Thus the fluid, though freely flowing out, did not escape into the peritoneal cavity. This cyst having been completely emptied, I introduced

my finger and broke up several smaller cysts, emptying their contents into the one common sac. Each cyst seemed to contain different contents, some an opaque whey-like fluid, in which curdy flakes were suspended; others fluid varying in consistence from clear serum to thick gelatinous substance; others, again, contained pure clotted blood. This having all been evacuated, the cavity was explored, and a large cyst was felt from the inside to be on the posterior aspect of the right side of the tumour, which was punctured by means of Cock's bladder trocar, but the fluid in it was of such a gelatinous nature that it would not flow. The cannula was removed, the opening enlarged, and by means of pressure on the parietes the contents were forced into the common cloaca already made. Again the finger was introduced into the cavity of the tumour, and more cysts forcibly broken up, which operation was now followed by an alarming flow of clotted blood. Presently this ceased, and finding pressure no more aided in reducing the bulk of the tumour, the orifice in the cyst was carefully closed by a double ligature. The next step consisted of enlarging the original incision through the parietes upwards and downwards, so as to make its total length four and three-quarter inches, its position being still midway between the umbilicus and pubes. The hand was then passed into the peritoneal cavity, in order to ascertain if any adhesions really were present. By this measure also was realized the full extent of the tumour, the surface of which guided the hand up underneath the diaphragm, below the free edge of the liver, and into each iliac fossa; except in front, no adhesions could be detected. An enormous cyst filled up the left hypogastric region. The tumour was rotated so as to make this cyst present at the abdominal incision; it was then opened into by the bladder trocar. Unfortunately the vulsellum used to hold the tumour in this position having been unduly dragged upon, ruptured the cyst wall, and quite unexpectedly deluged the whole surrounding structures with its contents, some of which unavoidably escaped into the peritoneal cavity. Through the ragged opening made in this manner several other cysts were broken up with the finger, and evacuated of their contents, which still presented the same miscellaneous characters just alluded to. Though the tumour had now been relieved of about eight pints of fluid, it appeared yet too large for extraction through the incision that had been made. The second orifice in the tumour was now ligatured, and I changed my position to the left side of the patient. The

left hand was passed all round the tumour, to further make sure that no adhesions were present that could prevent its removal. Gentle traction was now made upon the tumour, which elongated itself and partly protruded. An assistant once more essayed to use the vulsellum, and ruptured another large cyst, the contents of which were emptied externally—as the partly protruding tumour now completely blocked up the wound in the peritoneum—the whole bulk was now so lessened that the tumour was with slight difficulty coaxed out. It was attached to a narrow pedicle about three inches in length; this was secured in the usual manner by a curved clamp, and the tumour cut off. The peritoneal cavity was carefully sponged out, and the wound closed with five deep and five superficial sutures of fine pliable iron wire. The operating couch was placed next her bed, and the patient carefully lifted into it—a slip of lint damped with carbolic lotion (1 to 40) was placed on the wound; a many-tailed flannel bandage adjusted round the body, and twenty minims of tincture of opium administered. From the first inhalation of chloroform to twisting the last suture, exactly an hour and a half elapsed, fully an hour was occupied in extracting the tumour through the small incision made.

For some time after the operation there was great pain in the bowels and around the region of the wound, which the tincture of opium subdued; there also was considerable depression due to the great quantity of chloroform necessarily used during the prolonged operation. About an hour after being put to bed she complained of chilliness and cold extremities. Hot jars were applied—hot brandy and water given, and a second opiate draught administered.

At the fourth hour after operation the patient was quiet, with occasional pain through bowels; thirst was relieved by iced water; she complained of an aching pain in back, and twenty minims of tincture of opium were given, but were rejected; the vomiting causing the wound to become painful, lint saturated with tincture of opium was applied to it and gave almost instantaneous relief; pulse 104; weak.

2nd day, 27th June, 5 a.m.—Passed quiet night. Has been given iced water, and iced brandy and water during night. 8 a.m.—Water drawn off; sp. g. 1020, acid reaction, no albumen; pulse 105; quiet and tranquil. 12 noon, pulse 112; no pain. Has had tea, toast, jelly, brandy, and iced water. Hiccough rather troublesome; water to be drawn off every four hours. 2 p.m.—Had a few spasms of pain about region of wound, which was relieved by tincture of

reg. 4 p.m.—Complained of flatulent pains all over which were relieved by an opiate draught; has unpleasing at region of kidneys. 6.30 p.m.—Pulse 115; temp. of ward, 65°. 9 p.m.—Flatulent pain in abdomen troublesome, and again relieved by opiate. 12 midnight.—Quiet; pulse 108.

3rd day, 28th June, 5 a.m.—Pulse 120; temp. $99\frac{4}{10}^{\circ}$; temp. of ward 63°; slept well during the night; has been fed on iced brandy and water and beef-tea. 9 a.m.—In calm slumber; no observation. 1 p.m.—Pulse 108; temp. $98\frac{4}{10}^{\circ}$; temp. of ward 66°; carbolic acid renewed over wound. 6 p.m.—Having been injudiciously supplied with jelly by a temporary nurse, slight vomiting was induced; diet during day consisted of toast, jelly, iced brandy and water, and tea; the urine only required to be drawn off three times during the day, at 7 a.m., 2 p.m., and 8 p.m.; sp. g. 1022; no albumen; quantity of mucus, acid at first, but rapidly becoming alkaline. 6.40 p.m.—Pulse 118; temp. $98\frac{8}{10}^{\circ}$; temp. of ward 68°; perspiring freely; complained of being hot; got fire in ward reduced; flatulence troublesome, relieved by aqua menthæ piperitæ 7.35 p.m.—Flatulent pains all over abdomen, relieved by m. xx tinct. opii. 12 midnight.—No observation; in calm slumber.

4th day, 29th June, 5 a.m.—Pulse 112; temp. $99\frac{4}{10}^{\circ}$; temp. of ward 63°; slept well during the whole night, only waking occasionally and taking nourishment, then dropping off to sleep again. 8 a.m.—No observations; in calm slumber. 12 noon.—Pulse 115; temp. $100\frac{1}{2}^{\circ}$; temp. of ward 64°; water was drawn off at 9.30; sp. g. 1024; no albumen, high coloured, containing pus, and copious deposit of lithates on cooling, acid when first drawn, rapidly becoming alkaline; had a slight watery evacuation from bowels; iced brandy and water, toast, and jelly for nourishment. 2.30 p.m.—Pulse 104; temp. 99°; temp. of ward 64°; quiet; no pain; skin moist; slight flatulence. 6.30 p.m.—Pulse 103; temp. $98\frac{8}{10}^{\circ}$; temp. of ward $64\frac{1}{2}^{\circ}$.

It is not necessary to quote the daily notes of the case which were taken after this date. The patient was nursed with great care, and her diet carefully regulated; the wound was dressed with carbolic dressings; the urine regularly drawn off for the first six days, and nervous symptoms allayed by the bromide of potassium, and other sedatives. On the 23rd July, the patient was discharged from the Hospital, and on August 4th menstruation occurred; it continued three days, and was unattended with suffering.

Observations.—When I was first called to see this case I desisted from making a vaginal examination, in compliance with a principle that I have established in my practice, to avoid doing so in the case of an unmarried female, until every other legitimate means of diagnosis has failed. Though this course delayed positive diagnosis for about two months, the tentative treatment adopted during that period was productive of good results, as the steps taken to improve her general health, in order to restore the menses, were daily placing the patient in the best possible condition to bear the operation of ovariectomy, should certain suspicions as to the nature of the case be verified upon ulterior and more searching examination.

In deciding to extirpate the tumour, I considered the following to be in favour of that course:—1. Great moral courage and hopeful disposition of the patient. 2. Her age, twenty-nine years. 3. Probable absence of adhesions. 4. Favourable state of her health. 5. Favourable meteorological condition of atmosphere at the period of the operation. With reference to the two first, remarks would be superfluous. Concerning the third, the question of adhesions, I placed great reliance on the regularity of the excretions from the rectum and bladder, and the docility with which they responded to medicinal treatment, all of which were under careful observation from the commencement of my attendance. Madam Boivin, in her researches on the causes of miscarriage, found that adhesions of the uterus with the parietes or viscera caused that organ prematurely to discharge its contents. Following an analogical line of argument, I felt confident if there were adhesions existing between this tumour and the urinary bladder that there would be a disturbance in the functions of the latter, either a frequency or some irregularity of micturition. Likewise adhesions with the intestines could not but have interrupted their peristaltic action, the result of which would have been exhibited by some eccentricity of the faecal excretion. The liver, pancreas, and kidneys, all apparently performed their functions satisfactorily, to satisfy myself concerning which the usual qualitative analytical observations of excreta were made. Digital examination per vaginam demonstrated the uterus, though displaced and pressed backwards, to be quite movable and non-adherent to the tumour. The patient's condition of health for bearing the operation was everything that could be desired. The situation of the hospital on an elevation overlooking the Pacific Ocean, and the most seasonable state of the

weather, placed the patient in a most favourable hygienic position for recovery. For European readers I append meteorological observations taken daily during the course of the case.

Observations on the Operation.—In order to satisfy myself what would be the smallest incision through which could be extracted a tumour such as possessed the peritoneal cavity of the patient, I constructed a model multilocular tumour out of several bullock's bladders, which I filled with thick paste, and secured within the cavity of a rudely-constructed dummy by numerous adhesions. I covered this over by different layers of textile material to imitate the abdominal parietes. I thus not only secured for myself a most instructive rehearsal of the operation, from which I derived many useful hints, but satisfied myself completely that, by the exercise of patience and a moderate amount of dexterity, a multilocular tumour of large size, and with moderate adhesions, could be extracted through an incision about four and a half inches in length. I therefore commenced the operation with the intention of extracting the tumour through the smallest possible incision, and cannot but attribute the most favourable recovery that my patient made to my rigid adherence to this resolution.

The strong convictions that I have on the subject of the short incision are supported by the following considerations:—1. The shock of operation is lessened. 2. Decreased risk of extensive peritonitis. 3. Decreased risk of complications from protrusion of intestines. 4. The wound heals more readily.

1. *The Shock of Operation is Lessened.*—In a recent controversy, the small-bore *versus* the large-bore rifle, it has been elicited by observation on deer and other lower animals of a larger class that the larger the projectile with which the animal is pierced the greater the shock and the more sudden the death. A deer pierced through a vital part with a Whitworth bullet will run a considerable distance, while another pierced with an Enfield bullet (of nearly double the diameter) through a less vital part falls at once. I cannot see any objection to applying the same rule to wounds made by the surgeon's knife in such an important structure as the peritoneum. It may be argued that the handling, pressing, and squeezing, as well as the prolonging of the operation, more than counteracts the utility to be derived from the short incision. Against this I would submit that the peritoneum has been for months, often years, carrying within its cavity a tumour of considerable weight, and has been, as it were, educated to tolerate a

certain amount of rather rough usage. As to the prolonging of the operation, the result of my own observation, in several cases of ovariectomy that I have witnessed, has been a daily increasing conviction that, in these days of anæsthetics, it is rather an advantage than otherwise. Where there is danger of hæmorrhage rapidity of execution would be of course absolutely necessary, for thereby blood is saved; but in such an operation as ovariectomy, where hæmorrhage is comparatively trivial, and bleeding vessels can be stopped as they spout, to my mind no such necessity exists, and I think another illustration from experiments on lower animals may suggest that the very slowness of the operation might possibly the better adapt the system to bear it. I allude to those experiments on the living brains of lower animals where, by suddenly plunging instruments into the substance of the brain, death was instantaneously produced, but it was found that an equal injury was compatible with life if gradually and slowly inflicted.

2. *There is less Risk of Peritonitis.*—That is by extension of the adhesive inflammation necessary for the healing of the wound. In a structure like the peritoneum, where inflammation once set up is so liable to spread by continuity of surface, this is a most essential consideration. The inflammation necessary for healing an incision four and a-half inches long is serious enough, but every inch beyond that must increase the risk in a ratio almost approaching geometrical progression.

3. *There is less Risk of the Intestines Protruding.*—This was amply illustrated by the case under consideration. Throughout the whole operation the tumour was kept before the abdominal incision by pressure on the sides of the parietes, and formed a mechanical obstruction to the protrusion of viscera.

4. *The External Wound Heals more readily.*—It must be remembered that the incision is made through a structure, virtually a point of insertion of the oblique and transverse abdominal muscles, with the rectus abdominis situated at each side of it; all these muscles are thrown into violent action by the process of vomiting, an almost constant sequence to the operation; therefore, the longer the incision in the linea alba, the more difficult is it to keep the edges of the wound in that proper coaptation necessary for speedy union.

Difficulties met with during the Operation.—These can be briefly summed up. The trocar not properly acting; the tumour being multilocular, and the difficulty experienced in getting at, and breaking up the numerous small cysts; the smallness of the incision,

compared with the size of the tumour; chloroform sickness supervened in the midst of the work of breaking up the cysts, and for the time distracted our attention; the rottenness of the cyst wall caused it to rupture twice when traction was made on the tumour, and this was the means once of admitting some cystic fluid into the peritoneal cavity.

The Tumour.—The whole left ovary was found to be engaged in the disease. The tumour in fact consisted of an enormous hypertrophied ovary. It weighed, after extraction, six pounds, and about ten pints of fluid were evacuated previously in order to lessen its bulk. It properly belongs to that class of ovarian tumours known as proliferous, consisting of a multiplicity of hypertrophied dropsical Graafian follicles, in various stages of morbid growth; four large cysts exceeded all the others in size; three of these were evacuated in the course of the operation, the fourth was ruptured by making traction with a vulsellum when the tumour was in course of delivery through the abdominal incision. The pedicle was formed of the ligament of the ovary, the Fallopian tube and broad ligament carrying blood vessels for the nourishment of the tumour. A point of special pathological interest in this tumour is the fact that the fimbriated extremity of the Fallopian tube was agglutinated to the tumour by its posterior surface, so that the aspect of the free end, which at ovulation should be in apposition with the ovary in order to grasp the ovule, was looking away from the tumour.

Pathological Observations.—The peculiar morbid relationship here found existing between the Fallopian tube and diseased ovary, is very suggestive for future pathological inquiry. No data are derivable from an examination of the tumour to elicit whether the agglutination was antecedent to, or the result of the hypertrophy of the ovary. I venture to hazard the following theory:—The history of a long standing left-side pain would point to chronic ovaritis, extending to the peritoneal coat of the ovary, as a result of which adhesions took place between the Fallopian tube and the ovary; the continual juxtaposition of the fimbriated end of the tube with the ovary excited that organ to rapidly proliferate and mature its Graafian vesicles and hurry them towards the exciting point; the mal-apposition of the adherent tube prevented their discharge in the usual manner, so they, remaining in the ovary, took on vital action and hypertrophied, each one as it grew excited others, the vascularity of the structures increased, ultimately the whole

ovary became implicated, and the disease followed the usual course; those cysts nearer the surface, and meeting with least resistance, attaining the greatest size.

Thermometrical and Meteorological Observations.

Date	Hour of day	Days from Operation	In the Hospital Ward			At sea coast 112 feet above the sea level			Wind		For 24 hours previous to 9 a.m.		
			Pulse	Temperature in Axilla	Temperature of Ward	Barometer	Temperature in Shade	Humidity	Direction	Force	Maximum	Minimum	Rain in inches
1872													
June													
" 26	12.30 p.m.					29.959	54.7	74	W.	0.0	65.8	49.8	0.0
	2												
	6.30 "		120	98.8	63								
	6.30 a.m.	2	115	101.4	65	30.028	53.2	79	W.	0.7	65.3	45.3	0.0
	midnight		108										
28	6 a.m.	3	120	99.6	63	29.891	57.7	86	N.W.	1.3	62.3	52.3	0.07
	6.40 p.m.		118	98.8	68								
29	6.25 a.m.	4	112	99.4	63	30.009	56.2	83	W.N.W.	1.1	68.8	50.8	0.0
	6.30 p.m.		108	98.8	64								
30	6.30 a.m.	5	104	98	62								
	5.45 p.m.		104	100.2	66								
July 1	6.45 a.m.	6	110	100.6	63	29.792	55.7	69	W.N.W.	1.6	67.3	51.3	0.0
	midnight		94	98.2	65								
2	7 a.m.	7	103	97.7	62	29.713	54.7	74	W.N.W.	1.1	62.3	50.3	0.0
	7.30 p.m.		120	100	64								
3	8 a.m.	8	118	100.8	61								
	6.45 p.m.		114	101.4	63								
4	8 a.m.	9	124	100.2	62	30.225	46.2	81	N.W.	0.0	59.3	43.3	0.0
5	8 "	10	112	98.8	58	30.225	47.2	81	W.	0.4	61.3	41.3	0.0
	2 p.m.		120	104.6	62								
	5.45 "		128	102.6	62								
6	8 a.m.	11	88	99	62	30.426	51.7	99	W.N.W.	0.1	63.3	46.3	0.25
	6 p.m.		98	99.5	63								
7	8 a.m.	12	96	100.6	63								
	5.45 p.m.		112	101.4	63								
8	8 a.m.	13	100	101.8	59	30.212	45.7	99	N.W.	0.1	63.8	42.8	0.0
	5.45 p.m.		108	99.0	64								
9	8 a.m.	14	100	97.8	58	30.067	47.7	99	W.N.W.	0.6	63.8	44.8	0.0
	6 p.m.		103	101.4	63								
10	6 a.m.	15	100	99.2	59	29.886	46.2	95	N.W.	0.2	59.8	45.2	0.0
	6 p.m.		100	98	60								
11	8 a.m.	16	100	99.2	60	29.868	54.2	71	W.N.W.	2.9	61.3	48.3	0.0
	6 p.m.		104	99.1	64								
12	8 a.m.	17	121	97.7	60	30.002	55.2	69	W.	1.1	62.3	48.3	0.0
	2.30 p.m.		92	98.5	64								
13	8 a.m.	18	100	98.2	61	29.956	53.7	78	W.	0.6	63.8	50.8	0.0
	6 p.m.		108	98.4	63								
14	8 a.m.	19	95	99.1	62								
	7.45 p.m.		110	100.3	64								
15	8 a.m.	20	100	99.2	61	30.025	51.7	73	W.S.W.	0.8	67.8	44.3	0.0
	6.15 p.m.		118	101.3	62								
16	9.30 a.m.	21	100	99.2	62	30.221	55.7	74	S.W.	1.4	68.3	47.3	0.16
	6 p.m.		96	99.8	62								
17	8.30 a.m.	22	80	98.7	61	30.215	49.2	99	S.W.	0.8	61.3	48.3	0.15
	6 p.m.		98	99.1	61								
18	8 a.m.	23	78	98.6	59	30.065	52.7	73	W.	0.1	54.8	45.3	0.03

In the above table the object has been to record the greatest diurnal variation of the temperature in the right axilla; but two observations are given, out of several that were taken daily, each of those given represents respectively the highest and lowest points of variation. It was found convenient also to make notes of the temperature of the ward, not so much for scientific use as to impress the attendants with vigilance and stimulate them to keep the ward in as equable a temperature as possible.

At 2 p.m., 5th July, it will be noticed that an alarming increase of temperature, of nearly six degrees, took place in the patient's axilla since 8 a.m. that morning; this resulted from the decaying pedicle, partially mummified, having sunk into the lower part of the wound, where it remained surrounded by, and bathed in a quantity of discoloured ill-smelling purulent discharge. Though at 8 a.m., the pedicle was promptly removed with a scissors, and the discharge sopped up with wadding, it was not until towards the evening that the temperature decreased; and there is no doubt that a serious complication was happily averted, which we would have been totally unconscious of had it not been for the timely warning given by the thermometer.

The meteorological observations will prove interesting to European readers, showing as they do the favourable hygienic condition of our Australian midwinter.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Manual of the Diseases of the Eye. By C. MACNAMARA, Surgeon to the Chadnie and Ophthalmic Hospital, Calcutta; Professor of Ophthalmic Medicine and Surgery in the Calcutta Medical College. Second Edition. London: J. & A. Churchill, New Burlington-street.

THE very favourable judgment we pronounced on Professor Macnamara's admirable work in our number for February, 1870, has been fully borne out by the rapid sale of the original edition.

We can with truth accord a still greater meed of praise to this second edition, which we welcome as the best manual on the subject. In this new edition the very few errors of the original have been corrected, much of the work has been re-written and condensed, and new matter has been introduced. Several of the coloured plates, from Dalrymple's magnificent but little used work, have been omitted, and the series of ophthalmoscopic drawings has been judiciously curtailed and greatly improved by the substitution of better plates than those in the first edition.

Amongst other matters to which we referred at considerable length in our original notice of this work, was the employment of sunlight for ophthalmoscopic purposes. The author was of opinion that "the advantages of sunlight are so great that it should always be used if practicable." From the fact of all reference to this matter being omitted in the second edition, we infer that further experience has not confirmed his former conclusions.

We cannot conclude this notice without again repeating our oft-expressed sentiments on the operation of reclination or depression of cataract, which is a most dangerous and unjustifiable operation; it should, as a rule, never be practised, neither should it find a place in scientific works, except for the purposes of historical record and of condemnation. Although he describes this operation, Professor Macnamara bears ample testimony against its performance, for he

states that scarcely a week passes in which he does not see several cases of choroiditis and retino-choroiditis as the result of this reprehensible practice amongst the natives of India.

On Affections of the Heart and in its Neighbourhood: Cases, Aphorisms, and Commentaries. By HORACE DOBELL, M.D.; Senior Physician to the Royal Hospital for Diseases of the Chest. London: H. K. Lewis, 136, Gower-street. 1872. Pp. 128.

THE work before us consists of a number of cases (fifty-eight) culled from hospital and private practice. In detailing these, abstruseness is, as expressly declared, avoided, and there is the result of much intelligent observation recorded in them. Deductions from these cases are also given, which Dr. Dobell is pleased to dignify by the name of aphorisms. There is, however, a want of completeness about the majority of them, as the symptoms and diagnosis could not be brought to the crucial test of an autopsy. This volume is but the first instalment of a more extended work on the same subjects—the parachute that tests the wind for the balloon.

There are three illustrations. The first, an excellent one, a reproduction, by the Heliotype process, of Dr. Lee's drawing of the heart, with the aorta and nerves. The second is an indifferent and grotesque photograph of clubbed finger-ends, and the third is a wood cut of a heart bed, a description being attached. Dr. Dobell hopes that this bed "may prove an unspeakable boon both to patients suffering from severe heart disease, and to those anxious friends and attendants weary of witnessing their unalleviated woes."

"In order that this bed may be obtainable at the smallest practical expense, and therefore as widely useful as possible to all classes, I have not reserved to myself any patent right or interest in the invention, and I trust that no one else will limit its use by doing that from which I have purposely abstained." We trust that such philanthropy will meet with its reward somewhere.

Dr. Dobell seems to lay considerable stress on the occurrence of pain in the throat as a symptom of evil augury in cases of cardiac disease. We are not disposed to consider it of much importance. It occurs quite too rarely to entitle it to a place in the symptomatology of heart disease, and the *rationale* of its production is not explained.

On the Functional Diseases of the Renal, Urinary, and Reproductive Organs, with a General Review of Urinary Pathology. By D. CAMPBELL BLACK, M.D., L.R.C.S., Edin.; Member of the General Council of the University of Glasgow; Author of "Observations on Therapeutics and Disease," "On Certain Points in the Pathology and Treatment of Gonorrhœa," "On Syphilitic and Phagedænic Ulceration," etc., etc. London: J. & A. Churchill. 8vo, pp. 321. 1872.

THIS book consists of a preface, seven chapters, an appendix, and an index. In the preface the author states that it was his intention, on applying himself to the task embodied in the book, merely to have supplemented a paper by Dr. Bradbury, of Cambridge, which appeared in the *British Medical Journal*, 8th April, 1871, entitled "Clinical Remarks on the Treatment of Nocturnal Enuresis and an Allied Affection."

"As I reflected on the literature of the subjects treated of, and brought to bear upon it the result of not a little attention on my own part, my undertaking assumed proportions which placed it beyond the limits of medical journalism, and thus necessitated its appearance, if at all, in this form. Besides, the subject of 'nocturnal enuresis,' as it has been euphemistically called, 'an allied affection' (the subject of Dr. Bradbury's paper), appeared to me to present, both pathologically and physiologically, such close analogies to the other affections of the genito-urinary organs, that their relative interdependence and importance would be better exhibited by being considered in conjunction with the other functional disorders, an undertaking which I have consequently hazarded, more especially in the light of the opinions I have enunciated in my pamphlet 'On Therapeutics and Disease,' and elsewhere."

Chapter I. is headed "On the conditions that affect the Secretion of Urine, with special reference to suppression." In the section of this chapter devoted to suppression from congestion, inflammation, and other diseases of the kidneys, the following passage occurs (p. 58):—

"Congestion of various organs, it may be submitted as a rule, is a condition usually seen by fools. At least, my experience has taught me to attach little importance to the discrimination of the frequent discoverer of 'congestions.' How there can exist congestion of any organ, independent of static conditions on the one hand, and inflammation on the other, I have never been able to determine. Yet, like every other

conceivable and not a few inconceivable diseases, congestion is frequently diagnosed by a process of reasoning to me equally inexplicable."

Three lines further on:—

"But that congestion of the kidney does occur, under particular circumstances, it would be folly to deny."

We confess ourselves to be one of those *fools* who frequently see or meet with congestions of the brain, lungs, and liver, as well as of the kidneys.

The author appears to be almost as sceptical of the existence of such a thing as medical science as he is of congestion, for this chapter winds up with a disquisition on the subject:—

"What is medical science? A few—a very few—ultimate facts; a chaos of isolated assertions and contradictions, innocent of the parentage of reason, but greedily accepted, according to the law of supply and demand, by a gullible public; ideas selected from the confusion of untutored brains (for that is of no matter, or rather, it is an advantage), as fancy and the purpose to be subserved determine; a system of introducing into human bodies heterogeneous compounds and mixtures, of which little, to affect changes of which less, is known; removing substances when they are believed to exist in the blood coincident with particular symptoms, and introducing them when they do not—a conflict in which nature often conquers! Like the schoolboy running with his heart in his throat over the treacherous quagmire, the modern physician is unable to find a firm footing in the medicine of latter days, and his dalliance with remedial (?) applications, and endless experiments on frail humanity are comparable simply to the relations which subsist between the 'fast' young man and his tailor, or the 'girl of the period' and her accommodating milliner. But medicine *does* present a field for scientific investigation—a scope for genius.—Yes! there is such a thing as *medical science*, but there is likewise such a thing as *medical trade*; the few devotees of the former occupy the garrets of the profession; the many of the latter rollick away useless lives in affluence and luxuriance! Quackery abounds!"

Truly if medical science be what it is here represented, its devotees have no right to occupy any place but the garrets. In this chapter we did not observe any mention of the digitalis poultice to the loins in suppression of urine.

Chapter II. deals with "Retention of Urine; its varieties, causes, and treatment." On the subject of retention of urine from stricture of the urethra the author takes Sir Henry Thompson (p. 134)

severely to task for his opinions as to the existence of spasm and spasmodic stricture in the urethra.

Chapter III. discusses, in six pages, irritable bladder and strangury.

Chapter IV. is the central and essential one in the book; it is headed "On the Pathology and Treatment of Nocturnal Enuresis and Spermatic Incontinence." Six pages are devoted to the former subject and one hundred and nine to the latter, or cognate matters. A new term, spermorrhagia, is coined, p. 164, and the author suggests in a foot-note—

"That the term spermatorrhœa be at once and for ever expunged from medical literature. Besides being a misnomer, it has an abominable ring. Spermorrhagia more aptly expresses the meaning of the term which should be employed."

We are reminded that spermatic incontinence is the stock-in-trade of quacks, professional and non-professional, and it is remarked that "there seems now to be a laudable inclination on the part of legitimate practitioners to enter the charnel-house of quackery, and explore in every direction to what extent the superstructure is based upon truth and reality." The following questions are there discussed:—Do the testicles constantly secrete semen? Can the discharge of the peculiar secretion of the vesiculæ seminales, the prostate, and Cowper's glands, take place independently of one another? Are the vesiculæ seminales merely the reservoirs of the seminal fluid? Is semen re-absorbed into the system? The propriety of castration and clitoridectomy for epilepsy is considered, and in allusion to one of the particular advocates of the latter operation the following paragraph occurs (p. 288), both the spirit and style of which, in our opinion, would be a great disfigurement to any medical work, no matter what its intrinsic merit might be:—

"Human nature! given a man down, how universal the kicking! Every rogue passing by stops to take his turn. The pastime is characteristic of certain sections of all professions. Would that I could draw aside the veil which obscures the worse than Paphian temples, where the votaries of instrumental prostitution crowd to worship. Here one 'observer' taking the attitude of an ulcer of the os, which has existence only in his own brain; there another dangling the uterus on a 'staff,' like a hare-bell on a herd-boy's sprig; but these gentlemen are 'scientific' practitioners of medicine, and as such are applauded!"

As in page 288, we have the author saying:—

“I have actually never seen an ulcer of the uterus, that invaluable stronghold of the ‘Phalloid School,’”

we think he might concede to others a little more willingly and gracefully the latitude of observation which, in this part of the book, he claims for himself, in reference to the attractive subject of “anticipating misemission,” and where he says:—

“Dawson’s book, Acton’s book, Milton’s book, *et hoc genus omne*, contain much, in my opinion, that is overdrawn, irrelevant, and consequently pernicious.”

To some the use of *hoc genus omne* would suggest the proverb about the dwellers in glass houses.

Chapter V. is on “Sterility in the Male.” Chapter VI. on “Male Impotency.” Chapter VII. (seven pages in length) discusses “Anomalous Urethral Discharges.” The appendix consists of a letter from the author to the *Lancet*, and two extracts from that Journal elicited thereby. We were surprised to notice in the index that the derivations of such well-known terms as catarrh, cirrhosis, nephritis, hypertrophy, phymosis, hæmorrhoids, &c., are given, since from the opening paragraphs of chapter VI. we should not have suspected the author of having such a highly cultivated classical taste. We would have liked a much more accurate form of quotation than is generally used in the book: it is no great trouble to give page, volume, year, and exact title of the work whence the information is derived, and it is a great satisfaction to a numerous class of readers. We may select as an illustration of this want, the quotation (p. 189) of the gratifying observation of Budge upon the old cat whose testicles showed *post-mortem* movements upon irritation of the cerebellum with caustic potash.

We regret to be unable to say that we have derived either profit or pleasure from the perusal of this work, nor are we even impressed with the conviction that it will take a high place among books of its class, marred as it is by an undignified style and intolerance of opinion.

Irregularities and Diseases of the Teeth. By HENRY SEWILL.
London: Churchill & Sons.

MR. SEWILL has concisely and clearly indicated the general characteristics of irregular teeth, and has supplied illustrations of

the more common examples, together with the mode of treatment likely to be of use to the general practitioner. Irregularities of the teeth tend to increase the premature decay of these organs, and generally cause great personal disfigurement and inconvenience to the patient. Hence judicious treatment is of the highest importance, as by it they may be cured or prevented; while, on the other hand, unnecessary meddling leads to suffering and injury. Some interesting cases are given of facial neuralgia, which had resisted previous treatment, being permanently cured by the extraction of teeth whose roots were extensively exostosed. Some remarkable cases of indigestion, ulcer of the tongue, necrosis of the jaw, and abscesses in connexion with the teeth, show the necessity of having examinations of the teeth and mouth performed by skilled persons.

On Deformities of the Mouth, Congenital and Acquired, with their Mechanical Treatment. By JAMES OAKLEY COLES. London: John Churchill & Sons.

THAT operative or mechanical treatment for such a deformity as congenital cleft palate should have attracted the attention of surgeons for the last three hundred years is not to be wondered at, and Mr. Coles has done well in collecting and arranging in a careful and interesting manner all the latest facts that throw light on the mechanical treatment of the deformity. Cleft palate, as far as Mr. Coles' observations have extended, is not hereditary, and the lessening in width of the fissure, as observed between childhood and old age, shows that there is some effort on the part of nature to correct the deformity. Mr. Coles has made an interesting discovery in connexion with these observations—namely, the growth of what appear to be nasal polypi, attached either to the anterior portion of the nares or the back of pharynx, either singly or in pairs—more generally the latter. The author has seen these growths in patients from six months to sixty-eight years. Chapter IV. contains an historical account of the appliances used for remedying cleft palate (whether congenital or acquired) from A.D. 1552 to the present time. The author gives the preference to mechanical treatment, which generally benefits the voice of the patient, does no harm to the parts, and leaves them in a more favourable condition (being less sensitive) for the surgeon, should an operation be thought necessary. He explains his views on operative measures as follows:—

"Much as I believe in the soundness of that system of curative dentistry introduced recently by Dr. Norman Kingsley, and followed out since by so many others besides myself, I am by no means wedded to my belief to such an extent as to undervalue the merits of the surgical operation for congenital cleft palate. I am fully prepared to admit that, provided the condition of the parts be suitable, the surgical operation, being done once for all, is superior to any mechanical contrivance that must necessarily be renewed from time to time. The question, therefore, turns on what is a suitable condition of the parts for operation. They may be very briefly stated :—

"(1.) A sufficient substance on each side of the cleft to admit of freely paring the edges.

"(2.) An amount of mobility that will admit of the free borders easily approximating.

"(3.) A sufficient length of the central portion of the cleft palate to produce perfect closure between the naso-pharyngeal cavities.

"I believe all physiologists and surgeons will agree with me that unless these conditions be present, it will be impossible, after the operation is completed, for the patient to acquire perfect speech. If the deformity is such that after the cleft is closed the free border of the soft palate cannot be brought into contact with the posterior wall of the pharynx, it is vain to expect that a perfect articulation will be attainable by the person operated upon."

The book is creditably got up, and profusely illustrated with seven lithographic plates and fifty-one woodcuts.

RECENT WORKS ON TEETOTALISM.

1. *The Fallacies of Teetotalism; or, the Duty of the Legislature in Dealing with Personal Freedom.* By ROBERT WARD, Editor of the *North of England Advertiser*. London: Simpkin, Marshall, and Co. 1872. 8vo, 414 pp.
2. *The Abuse of Alcohol in the Treatment of Acute Disease.* A Review. By J. P. HESLOP, M.D. London: J. & A. Churchill. 1872. Large 8vo, 39 pp.

THE authors of works and lecturers on teetotalism have, not unjustly, been accused of gross exaggeration of language, and perversion of statistics and even naked facts, when treating of those evils which they propose to remedy by total abstinence from "strong drinks." It is their object to show that the use of alcohol leads to the commission of most of the crimes which stain mankind,

besides being the cause of infinite domestic misery, loss of time, money, and rational enjoyment. Mr. Ward goes upon exactly the opposite tack, and endeavours to prove that all kinds of crimes and other evils are averted by a moderate indulgence in the cheerful glass. Although we think Mr. Ward pushes his theory of the benefits of alcohol too far, we consider his book a most able and convincing refutation of the fallacies put forward by the extreme teetotallers. At the present time, when "alcoholism" is a subject of great interest, Mr. Ward's book comes most opportunely, and especially so for the reason that whilst so many works have been published by the teetotallers, the opposite view has not been prominently put forward in the shape of a bulky volume like Mr. Ward's.

Dr. Heslop's work is a reprint of a review which appeared in this Journal in 1860. It advocates the moderate employment of alcohol in the treatment of disease, but shows that this agent has, in the hands of many practitioners, been much abused. The greater part of the *brochure* consists of a criticism of Dr. Dodd's *Clinical Lectures on Certain Acute Diseases*. Dr. Heslop conceives that this great physician erred greatly in over-stimulating his patients. Dr. Heslop's reprint makes its appearance at a proper time.

WORKS ON HYGIENE.

1. *On Diet and Regimen in Sickness and Health*. By HORACE DOBELL, M.D. Fifth and Revised Edition. London: H. K. Lewis. 1872. 8vo, pp. 196.
2. *On the Preservation of Health*. By THOMAS INMAN, M.D. Third Edition. London: H. K. Lewis. 1872. 8vo, pp. 402.
3. *A Treatise on Physiology and Hygiene*. By JOSEPH C. HUTCHINSON, M.D. New York: Clark and Maynard. 1871. 8vo, pp. 280.
4. *Air and Rain; the beginning of a Chemical Climatology*. By ROBERT ANGUS SMITH, Ph.D., F.R.S., F.C.S., (General) Inspector of Alkali Works for the Government. London: Longmans, Green, and Co. Large 8vo, pp. 600.

DR. DOBELL's work on "Food and Diet," though evidently intended for the edification of non-professional readers, is worthy of

perusal by members of his own profession. It contains a great variety of interesting information relating not merely to food, but also to many subjects of great hygienic importance. We can cordially recommend this little work.

Dr. Inman's work is avowedly written for the public and not the medical profession, but we do not agree with the author that his production will, on that ground, "be assailed by many who profess themselves members of a liberal profession." Many of the most distinguished, we might almost say orthodox, members of the medical profession, have written works on sanitary science and preventive medicine, and by so doing have not in the slightest degree forfeited the respect of their colleagues.

Dr. Inman's work is vigorously written, and contains a large amount of useful information relative to diet, clothing, bathing, exercise, dwellings, and every point affecting personal hygiene. It might, however, with advantage be pruned a little, for in a book intended for the use of the general public anecdotes relating to cases of *placenta previa* are somewhat out of place.

Dr. Hutchinson's little book is intended for use in schools and other educational institutions, as well as for those who wish to gain an elementary knowledge of the wondrous mechanism of the animal body. The author's language is clear and terse, and his descriptions, both anatomical and physiological, are singularly free from error. A large number of well executed wood-cuts add much to the value of the work. For use in schools we have not met so good a book of the kind with the impression of a British publishing house upon it.

Dr. Smith's "Air and Rain" may well be termed a classical work. It was referred to very fully in our Report on public Health in the November number of the Journal.

PART III.

HALF-YEARLY REPORTS.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.*

By WALTER G. SMITH, M.D., *Dubl.*; Fellow and late Examiner in Materia Medica, K. & Q.C.P.I.; Assistant Physician to the Adelaide Hospital.

- ART. 15. Absorption of medicines through the skin.
- „ 9. Alcohol.
 - „ 1. Arrow poison.
 - „ 10. Camphor.
 - „ 6. Carbolic acid and creosote.
 - „ 14. Chloral *v.* Strychnia.
 - „ 11. Conia.
 - „ 16. Cyanosis from nitrate of silver removed by iodide of potassium.
 - „ 17. Ergotin.
 - „ 3. Iodide of lead.
 - „ 12. Iodides of potassium and ammonium.
 - „ 4. Neutral sulphate of physostigmia.
 - „ 7. Opium.
 - „ 2. Pepsine.
 - „ 8. Phosphorus, poisoning by.
 - „ 18. Propylamine in Rheumatism.
 - „ 19. Sarsaparilla in syphilis.
 - „ 13. Therapeutics, further observations on.
 - „ 5. Tobacco.

* The author of this Report, desirous that no contribution to the subjects of Materia Medica and Therapeutics should remain unnoticed, will be glad to receive any publications which treat of them. If sent to the correspondents of the Journal they will be forwarded.

1. *On a New Arrow-Poison—The Kombé Poison of Africa.*—Dr. T. R. Fraser has made some interesting observations on this substance, and has re-published, with extensions, in the *Journ. of Anat. and Phys.*, Vol. vii., the abstract of his paper given in the *Proc. Roy. Soc., Edin.*, 1869–70. Of the two great groups of poisonous substances used in tropical countries—the *ordeal* and the *arrow-poisons*; we have, among the former, the *Tanghinia venenifera* of Madagascar, the *Physostigma venenosum* of Old Calabar, and the Akazga-poison of the Gaboon; and, among the latter, the famous Curara or Wourali of South America, and the *Antiaris toxicaria* of Java.

The plant from which the new poison is derived is the *Strophanthus Kombé* (Oliver), N. O. Apocynaceæ, and the greater number of the experiments were made with an extract prepared from the seeds, which was injected subcutaneously into frogs, birds, and mammals. The author believes that his preliminary investigation enables him to make the following statements:—1. *Strophanthus* acts primarily upon the heart, and produces, as the final result of this action, paralysis of that organ with permanence of the ventricular systole. In other words, it is a direct cardiac poison. 2. Pulmonary respiration continues in cold-blooded animals for several minutes after the heart is paralysed. 3. The striped muscles of the body are acted upon directly; twitches occur in them; their tonicity is exaggerated; and, finally, their functional activity is destroyed, the muscles being then hard, and, soon afterwards, acid in reaction. 4. The reflex function of the spinal cord is suspended soon after the heart is paralysed; but the motor conductivity of the spinal cord and of the nerve-trunks continues after the striped muscles of the body are paralysed. 5. The lymph-hearts of the frog continue to contract for many minutes after the blood-heart has been paralysed.

2. *Pepsine.*—So many failures have occurred with those who use pepsine that suspicion of the quality of the drug naturally arises. In the *Practitioner*, for June, 1872, the results of the examination of samples from three of the most eminent and respectable firms in London are given, and the inference to be drawn from them is not very gratifying.

No. I. Savory and Moore.—Pale brown powder, partially soluble in water; solution coagulated by boiling and gives a

precipitates with nitric acid, corrosive sublimate, and tannin; no starch present; possesses scarcely any digestive power.

No. II. Squire.—Pale yellowish powder, less soluble in water than I.; solution coagulates on boiling and yields precipitates with nitric acid, corrosive sublimate, and tannin; no starch; digestive power very feeble.

No. III. Morson.—Pale yellowish powder, slightly soluble in water; solution rendered only slightly turbid by boiling and gives scanty precipitates with nitric acid, corrosive sublimate, and tannin; contains 26% of starch, and possesses extremely slight digestive powers.

TABLE I.

	1	2	3
Moisture,	13·85	9·97	9·01
Organic matter soluble in water,	53·19	43·91	24·22
" " insoluble "	28·50	44·00	64·00
Mineral matters	4·46	4·12	2·77

In Tables II. and III. are given the results of the action of acidulated solutions of the specimens of pepsine on coagulated albumen (white of egg), and on fibrin (lean boiled beef).

3. *A New Solvent for Iodide of Lead*.—In the course of some experiments, Dr. D. Tommasi was led to the discovery that iodide of lead is freely soluble in solution of acetate of sodium. He finds that 50 c. c. of a cold concentrated solution of acetate of sodium will dissolve 1 gramme of the iodide; the same quantity boiling will dissolve more than 2 grammes. 50 c. c. of the same solution acidulated with $\frac{1}{2}$ of a c. c. of acetic acid, and boiled, will dissolve 6 grammes of the iodide; 20 c. c. of a boiling supersaturated solution, acidulated with acetic acid, will dissolve 8 grammes, *i.e.*, 40%. It appears to be a case of simple solution and not of the formation of any double compound. Chromate of lead, an occasional impurity in the iodide, can be readily detected by its comparative insolubility in sodic acetate.

It is well known that iodide of lead is insoluble in alcohol, ether, chloroform, acetic acid, and glycerine, and is almost insoluble in cold water (1 in 1,235 cold: 1 in 194 boiling), and hence the author suggests the introduction of the following preparation,

which might be termed a *glycerole*, as an efficient substitute for the officinal ointment:—*R.* Saturated solution of sodic acetate 15 c. c., glycerine 25 c. c., iodide of lead 0, 4 grm., rose-water, a few drops. Rub together in a porcelain mortar, and dilute with water if necessary.—(*Mém. présenté à la Soc. roy. de Pharm. de Bruxelles*, 1872.)

4. *Neutral Sulphate of Physostigmia (eserin).*—A definite solution of the pure active principle of Calabar bean is an evident desideratum to oculists, and, in the *Journ. de Pharm. et de Chim.*, a process is given whereby crystallized sulphate of physostigmia may be obtained. A convenient solution of this salt is one containing 1 cgm. of physostigmia to 1 grm.; and the formula, recommended as a collyrium, is 2 grm. of this solution to 8 grm. of water.—(*Pharm. Journ.*, Dec. 23, 1871.)

5. *Tobacco.*—MM. Vohl and Eulenberg (*Arch. Pharm.* [2] cxlvii., 130–166) have made some elaborate and valuable investigations on the physiological action of tobacco, with especial reference to the constituents of tobacco smoke, from which they conclude that the action of tobacco, when used as a narcotic, has been erroneously attributed to the nicotine it contains. The amount of nicotine in snuff they found to be only from 0.0392 to 0.062%; in the strongest tobacco for chewing there was only a mere trace of nicotine, and in other specimens of the same kind there was none at all. They then carefully analysed the smoke of strong tobacco containing 4% of nicotine, burning part of it in a pipe and part of it as cigars; and the most important result of their labours is that, instead of nicotine, not a trace of which was found, they obtained a whole series (no less than 8) of the picoline or pyridine bases, analogous to the aniline bases, ranging from pyridine, C_5H_5N , to viridine, $C_{11}H_{19}N$. The fact that stronger tobacco can be smoked in cigars than in a pipe is explained by the greater proportion of volatile bases present in the smoke of a pipe, and especially by the large quantity of very volatile and stupefying pyridine; while in a cigar little pyridine and much collidine is formed. These picoline bases possess a similar physiological action to nicotine, but do not act so quickly. Plants which contain no nicotine are not unfrequently used for smoking instead of tobacco, and the authors tested also the action of the pyridine bases produced from dandelion, willow-wood, stramonium, and of pure picoline from boghead coal. They, moreover, think that the action of opium, when smoked, is not due to the alkaloids it naturally contains,

and that the difference of its action from that of tobacco is due to a difference in the bases, which are produced when the two substances are smoked.—(*Pharm. Journ.*, Jan. 13, 1872.)

Dr. Heubel, of Kiew, contests, in the *Centralblatt*, the results of Zeise, Melsens, and Vohl, as to the non-existence of nicotine in tobacco smoke, for he has, apparently, succeeded in recovering nicotine from the smoke of a few cigars. He also shows that, although nicotine itself is highly volatile, its salts, tartrate, sulphate, &c., preserve their characteristic poisonous qualities when evaporated in the water-bath. Nicotine appears to exist in the leaf as malate and citrate; sulphide and cyanide of ammonium also exist in the smoke (Vogel and Reischauer).—(*Lancet*, Nov. 30, 1872.)

6. *Carbolic Acid and Creosote*.—(α) *Their mutual relations*.—The literature of creosote and carbolic acid is encumbered with so many contradictory and confusing statements that it will be worth while to advert to results obtained by Hlasiwetz and Gorup-Besanez, among many others, which have now cleared up the contested questions. In 1832 Reichenbach discovered true creosote in the distilled oils of beech-wood tar. Two years later, F. Runge announced the discovery of carbolic acid in coal-tar oil, and, for some time, these two bodies were believed to be identical. In 1855 another source of confusion was introduced, for in that year, cresylic acid (the next higher homologue of carbolic acid) was discovered by Fairlie in coal-tar, and, on account of its near resemblance to true creosote, was generally accepted as identical with that compound. Moreover, since carbolic acid, very shortly after its discovery, began to be sold under the name of creosote, it was difficult, and at times impossible, to procure the oils obtained from wood-tar; and properties have been attributed by chemists to creosote which really belonged to carbolic acid. We now know that two homologues are contained in that part of the oil of wood-tar which dissolves in caustic potash, and which bear a certain relationship to the two homologous compounds contained in the same part of the oil derived from coal-tar. Thus we have as the principal ingredients of,

COAL-TAR OIL.

Phenyllic hydrate (phenol) C_6H_5O
Cresylic hydrate (cresol) C_7H_7O

WOOD-TAR OIL.

Guaiacol $C_7H_7O_2$
Creosol $C_8H_9O_2$ } = creosote.

Hence, creosote is a mixture and not a definite compound.

The near relationship of these formulæ renders it easily credible that cresylic acid at the time of its discovery was thought to be identical with creosote.

When guaiacum is submitted to destructive distillation we obtain a fluid which, in many respects, bears a great resemblance to creosote, and, in point of fact, does contain guaiacol and creosol (homo-guaiacol). The boiling points of all these bodies are so close to each other that it is impossible to separate them by fractional distillation; but, their products of decomposition may be resorted to as a means of distinguishing between them, for, while phenol and its series yield, with nitric acid, nitro-phenol (picric acid) and similar compounds, we obtain, with guaiacol, oxalic acid.—(*Pharm. Journ.*, March 30, 1872, *N. York Druggists' Circ.*)

The blue coloration given by carbolic acid with perchloride of iron in *aqueous* solution, enables us to discover it when mixed with creosote; and, in the *Pharm. Journ.*, June 15, 1872, Professor Flückiger gives useful formulæ for making test-solutions; in the same journal Flückiger points out that the glycerine-test proposed by Mr. Thomas Morson (*Pharm. Journ.*, May 18, 1872), viz., that true creosote is insoluble, or nearly so, in glycerine, while carbolic acid readily mixes with it in all proportions, requires some modification. True creosote is perfectly insoluble in any proportion with *anhydrous*, or nearly anhydrous glycerine, but it is not so with a somewhat diluted glycerine. A clear solution of creosote with the same weight of anhydrous glycerine becomes turbid on addition of a little water, whereas a similar solution of carbolic acid may be diluted with water without separation of carbolic acid.*

(3). *Preparation of Pure Carbolate of Potash*.—According to M. Ramel this compound may be prepared by evaporating a mixed alcoholic solution of 94 parts of carbolic acid and 56 parts of caustic potash, or by fusing together 37.4 parts of caustic potash and 62.6 parts of carbolic acid, the potash being added gradually. The compound forms a crystalline mass of small, micaceous plates, soluble in water and alcohol, and with an affinity for moisture, turning first yellow and afterwards brown. The author states that this compound is being increasingly used in French medicine, and that, as a disinfectant, it advantageously replaces carbolic acid itself.—(*Pharm. Journ.*, Jan. 13, 1872, from *Bull. Soc. Chim.*)

* See a very full account of carbolic acid, its composition, properties, and uses, by Dr. J. H. Bill, in the *Amer. Journ. Med. Sci.*, for July, 1872.

(γ). *Action of Carbolic Acid and Creosote upon the Organism.*—Dr T. Husemann (*Amer. Journ. Pharm.*) discusses this important question, and, from the experiments made by Ummethun and others, concludes—1. Both creosote and carbolic acid exert a decided local and general action upon the animal organism. 2 and 3. The local and toxical effects of carbolic acid are more energetic than those of creosote. 4. The constitutional effects of both poisons are manifested by nervous disorders, but in a very different manner. Carbolic acid produces, at first, tremulousness, then spasms of such severity as is observed only from few poisons (picrotoxia, codeia); these continue for several hours, if the quantity of the poison was not excessive, and terminate in paralysis and death. The symptoms of creosote poisoning are different. At first there is considerable uneasiness, followed by much difficulty of breathing, and, in a short time, by prostration and paralysis, which often last for several hours before death ensues, but always without any signs of spasm. 5. Again, on *post-mortem* examination, the blood of animals poisoned by carbolic acid is always liquid; while, on the other hand, the blood of animals poisoned by creosote, is invariably characterized by an increase of its coagulability, and tough, hard clots were often observed in the heart and larger veins; never in carbolic acid poisoning.

Lastly, after death from creosote, hard circumscribed spots are found in the lungs; while, in carbolic acid poisoning, the lungs are usually found to be pale and shrunken, without any evidence of inflammation.

Although cases of indisputable creosote-poisoning in men are so rare, the case described by Müller, in *Wurtemb. Corresp. Bl.*, 1869, No. 42, exhibited effects corresponding to those observed in the lower animals.—(*Med. & Surg. Reporter*, Sept. 23, 1871.)

(δ). *Gangrene produced by External Application of Carbolic Acid.*—Numerous mishaps and even fatalities have occurred from the incautious or improper use of carbolic acid, both internally and externally, some of which have been recorded in previous *Reports*, but accidents are still too frequent to render warning superfluous. Thus, Dr. Tillaux has observed, three times within a month, at the St. Antoine Hospital, complete gangrene of the injured part (in two instances, a finger, and once, a great toe), follow on a faulty or ignorant method of applying carbolic acid (*Bull. Gén. de Thér.*, Sept., 30, 1871). M. Poncet has also twice seen, in M. Ollier's wards, gangrene of the finger follow incautious dressing of a

wound with concentrated carbolic acid, applied before admission to hospital.—(*Bull. Gén. de Thér.*, Juillet 30, 1872.)

With these cases we may, with advantage, compare—

7. *Effects of Oil of Cade Externally Applied.*—Dr. Kirchheim relates the case of a man, aged twenty-three, suffering from eczema, for whom a mixture of juniper-tar (*huile de Cade*) and glycerine was ordered for external use. After he had applied it for about three days, he felt unwell, had headache, lost his appetite, and his urine was scanty, dark-coloured, and cloudy. These symptoms were followed by pain and tenderness in the region of the kidneys, œdema of the lungs, pneumonia, and albuminuria. He was treated with benzoin and warm baths, and with iron and quinine, and recovered after an illness of two months.—(*Brit. Med. Journ.*, July 6, 1872, from *Berlin Klin., Wochenschr.*, May 6.)

8. *Opium*—(α) *its Alkaloids.*—Many observers have followed in the wake of the well-known investigations of Claude Bernard on the opium-alkaloids, and the most contradictory statements have been published. The confusion has, in part, arisen from inferences being unjustifiably drawn as to the action of these complex bodies on man, from experiments made on the lower animals, although it is notorious that there is no class of substances which exhibits a greater diversity of operation on different animals.

M. Rabuteau, from numerous observations made on man, in a healthy and diseased condition, and on dogs, rabbits, and frogs, arranges the alkaloids of opium in the following order of activity, according to their effects *on the human subject*:—1. *Soporifics*—morphia, narceia, codeia. The others do not produce sleep. 2. *Toxic*—morphia, codeia, thebaia, papaverine, narceine, narcotine. 3. *Analgesics*—narceine, morphia, thebaia, papaverine, codeia. 4. *Anæsthetics*—Morphia, narceine. The others do not arrest diarrhœa.—(*Bull. Gén. de Thér.*, 15 Mai., 1872.)

M. Bouchut, in an elaborate paper (*Bull. Gén. de Thér.*, 15 and 30 Avril, 1872), relates a number of clinical observations made on children from three to fourteen years of age, and the more important of his conclusions are—In medicinal doses, none of the opium-alkaloids exercise a convulsant action. Morphia and its compounds are the most active and the most soporific of the preparations of opium. Codeia comes next, but it is necessary to give three times as much of it as of morphia to produce similar effects. As a

soporific, narceine ranks next, and considerable doses of it may be absorbed without producing sensible effects. Papaverine, when injected subcutaneously in doses of 10 cgm., produced no appreciable effect; and 1 grm., taken by the mouth, proved inert. Narcotine, thebaia, meconine, and opianic acid have no action upon the economy in doses of 30 to 50 cgm. Finally, for medical purposes, opium, first of all, then morphia, and next, codeia, can alone be employed with advantage.

(β). *Combined Action of Morphia and Chloroform.*—A short notice of Claude Bernard's observations upon this important point in practice, was given in the *Report* for February, 1870, and, guided by Bernard's experiments, M. Guibert has, during the past two years, utilized in man this association of morphia with chloroform. He has met with two distinct conditions, which are but degrees or stages in the action of chloroform, viz.:—1. Analgesia; 2. Anæsthesia. If the patient have already received a hypodermic injection of 1 or 2 cgm. of muriate of morphia, the first effect of chloroform, inhaled in the usual way, is to produce a state of analgesia, with preservation of the intelligence, of the senses, and of voluntary movement. If the influence of the chloroform be kept up, sleep is induced with anæsthesia, and relaxation of the muscles—the *mixed anæsthesia* of Bernard. MM. Labbé and Guyon also confirm Bernard's researches as applicable to man, and they think that, by this association, the risk of mortal accidents from chloroform would be much diminished.—(*Arch. Gén. de Méd.*, Avril, 13, 1872.)

M. Guibert calls particular attention to the stage of analgesia, not previously described, and has made a number of observations, principally on parturient women, and he suggests the following method as especially applicable to laborious deliveries. Let a subcutaneous injection of about 1 cgm. of muriate of morphia be given when the uterine pains begin to be severe. In about a quarter of an hour afterwards, at the commencement of a uterine contraction, chloroform is administered as usual, and, after a few inspirations, it will be found that the suffering due to the "pains" subsides, while the uterine action goes on. The analgesia thus produced greatly mitigates the torments of parturition and moderates the condition of profound fatigue which follows on a prolonged confinement. This method is evidently applicable to other cases, such as lead colic or any severe abdominal pain, &c.—(*Journ. des Conn. Méd.*, Juillet 30, 1872.)

Directly bearing on this subject are some experiments of M. Rabuteau. He injected subcutaneously into a dog 5 cgm. of muriate of narceine and then narcotised it with chloroform. When the animal awoke, although able to run about, it could be pinched or pricked without evidencing the least sense of pain, and, in fact, for several hours the sensitive nervous system was paralysed. Similar results were obtained when bromoform or chloral were employed in conjunction with narcotine.—(*Loc. cit.*)

(*γ*). *Case of Excessive Use of Morphia, hypodermically*—300 needles removed from the body.—Dr. Judson B. Andrews relates a very curious case, of which we can give only an outline. A single woman, aged thirty, was admitted into an asylum, in May, 1871, in a state of acute mania. She was of a nervous and highly emotional temperament, but with no hereditary tendency to insanity. From the age of twenty she was subject to frequent attacks of severe head-ache, attended with mental disturbance. Morphia, hypodermically, seems to have been first used for the relief of vomiting consequent on an attack of diphtheria at twenty-five years of age. Two years after this it was ascertained that she was in the habit of using hypodermic injections of morphia to relieve pain in her limbs and different parts of her body, and usually ran through \mathfrak{z} i. in a week. In one or two instances she had used as much as \mathfrak{z} ij. in a week. Both before and after she began the use of morphia, her conduct was peculiar and erratic. On admission, innumerable scars and marks of hypodermic injection were discovered all over the body, and probably, in all, about 2,000 injections had been practised within three and a-half years.

She gained ground, while in the asylum, for some weeks, but in July the right breast swelled to an extraordinary size, and, in August, an elevated spot, just under the skin, was noticed, from which, when cut down upon, a broken needle was extracted. Many other needles were removed from that breast, and later, from the left breast, the abdominal parietes, the mons veneris, labia, and vagina, some even running across the urethra and vagina. Needles were also removed from the legs, buttocks, and back.

About a month before death, localised pneumonia of the right lung set in, she lost ground, and died comatose on December 25th, 1871. Altogether 286 needles were taken from her body during life; 11 were found in the tissues after death; 3 were passed from the rectum during illness; making a total of 300. Of this number, 246 were whole, and 54 were parts of needles. In

regard to the presence of this large number of needles in the system, no information could be obtained from the woman herself, or from her mother. At the *post-mortem* examination, the arachnoid was found to be opaque and thickened over the right hemisphere, and the left hemisphere was covered by a thin layer of pus, contained in the subarachnoid space. Marked depression of convolutions at vertex of both hemispheres.—(Reprint from the *Amer. Journ. of Insanity*, July, 1872.)

8. *Poisoning by Phosphorus.*—(α.) *Physiological Action.*—According to Dr. Wegner, of Berlin (*Virchow's Archiv.*), the hæmorrhages which occur in all parts of the body from poisoning by phosphorus are due to an extension of fatty degeneration from the heart to the entire arterial system, and, in women, if the poison has been taken for some time previous to menstruation, it leads to profuse menorrhagia, and to the escape of much blood into the Graafian follicles, causing hæmatomata often of considerable size. Other bones besides the maxillary bones are probably liable to its destructive influence, but, from Wegner's experiments on dogs and rabbits, it appears that minute doses of phosphorus act as a specific stimulus upon the osteogenous tissue, for, if the very diluted vapour be allowed to act upon the exposed periosteum of a bone, it induces periostitis and consecutive hypertrophy of the osseous tissue. In a more concentrated state it may, in addition, cause suppuration of this tissue. If the animal be exposed to the vapour in a still greater degree of concentration, the action upon the osseous system does not advance beyond a certain point, and disorder of the digestive system becomes the most prominent symptom, while chronic indurative gastritis and interstitial hepatitis, with icterus, and ultimately cirrhotic atrophy of the liver result. These investigations suggest the therapeutic use of phosphorus in those diseases of children in which the activity of the osteogenetic processes is impaired, as well as in ununited fractures, sub-periosteal resections, and periostitic transplantation.—(*Lancet*, October 26, 1872.)

(β). *Treatment of Poisoning.*—The testimony in favour of the value of oil of turpentine as an antidote to phosphorus-poisoning is now very strong, as has been shown in the *Reports* for the past four years, and Dr. Andant, the originator of this treatment, contributes another successful case in support of it, the fourth in his practice. On the 2nd of January, 1871, he was summoned to visit a family, three of whom, Mrs. L. and her two sons,

had the preceding evening partaken of some bacon and cabbage soup, prepared in a vessel into which, as was afterwards discovered, a box containing 35—40 phosphorus matches had accidentally fallen. The younger son suffered but little, except from violent abdominal pains; the elder son was affected with burning thirst, and vomited twice; but their mother suffered more severely. She was unable to rise from her bed, was consumed with thirst, and towards morning she repeatedly vomited thready masses, which appeared luminous in the dark; she thought that she vomited fire. Next day the mother and elder son, especially the former, were very ill, and laboured under the most marked symptoms of phosphorus-poisoning, such as pains, cramps, debility, commencing icterus, scanty orange-coloured urine, and dysuria. The following prescription was ordered:—℞. Gum julep 100 parts, syrup of orange-flowers 20, oil of turpentine 4, tragacanth $\frac{1}{4}$. This was to be given to the mother and son four times, at intervals of a quarter of an hour; “eau albumineuse”^a to relieve thirst, and milk or other fatty body to be excluded from the dietary. Bile was found in the urine, both by the iodine test and other tests. The following day the bad symptoms continued, the same treatment was kept up, and the night passed off better. In two days more there was great improvement, and both were shortly able to resume their usual occupations, although still suffering from some debility.—(*Bull. Gén. de Thér.* 15th October, 1871.) So recently as 1867 Tardieu in his work on poisons asserted that there was no antidote, and that the cure of a well-marked case of phosphorus-poisoning was quite exceptional, but happily, this is no longer true. M. Rommèlère has lately presented a memoir, *De l’empoisonnement par le phosphore*, to the Academy of Medicine of Belgium, and an interesting abstract of the therapeutic part of it is given in the *Bull. Gén. de Thér.* 29th Fevr., 1872. The author relates three cases of attempted suicide by lucifer matches, in all of which the symptoms were very intense, and in all the treatment consisted essentially in the administration of turpentine, freely at first (60m.—75m.), and was followed by a favourable termination. He points out that the different modes of treatment recommended in acute phosphorus-poisoning may be classed under three categories:—

- I. To hinder the absorption of the phosphorus—(a.) By emetics;
- (b.) By dieting, i.e., avoiding all fatty substances, which favour

^a A solution of white of egg in water, aromatized with orange-flower water.

absorption of the phosphorus (Mialhe); (c.) By envelopment of the phosphorus with metallic copper (Bamberger). It is well known that phosphorus precipitates copper from a solution of the sulphate, and the idea was, first to give an emetic of sulphate of copper, and then to continue that salt in small doses. The suggestion is theoretically ingenious, but has never as yet been essayed in practice.

II. To prevent the effects of phosphorus on the blood—(a.) By inhalation of ether (Bellini); (b.) By alkalies (Bellini); (c.) By oxidants (Duffos); (d.) By transfusion of blood (Eulenburg and Landois).

III. Treatment by oil of turpentine (Andant).

None of the methods coming under class II. have any claim to practical value, but the utility of turpentine as a counter-poison seems now to be an established fact. As to the explanation of the antidotal power of turpentine, the theory of M. Personne (see *Reports*, August, 1869, and February, 1870), that phosphorus kills by hindering the hæmatosis of the blood, and that turpentine obviates this tendency, can scarcely be held any longer, especially in the light of the observations made by Köhler and Schimpff* (*Report*, August, 1871). These authors find that the efficacy of turpentine as an antidote rests on its forming with phosphorus a white compound resembling spermaceti (first described by M. Jonas), soluble in alcohol, ether, benzine, and the alkalies. It is termed *terebo-phosphorous* (phospho-terebic) acid, and can be obtained artificially (*Rep. de Pharm.*, Mai, 1872). This white compound is not formed with chemically pure turpentine, but only under the influence of the air, or of the ozonized oil (French turpentine), and this may explain why some experimenters, e.g., MM. Curier and Vigier, have been unsuccessful with turpentine, through employing the pure oil. 30 cgm.—1 grm. of this substance administered in alcoholic solution to dogs caused no inconvenience, and it is eliminated by the urine, to which it communicates a peculiar camphoraceous odour.

To sum up, the best treatment in acute phosphorus-poisoning consists in—1°. Giving an emetic. 2°. Administration of turpentine. 3°. Spare diet, free from fat. Nothing could be more inappropriate than the treatment adopted in a case reported in the *Lancet* for 1871, viz., a dose of castor oil and a free allowance of milk.—(*Bull. Gén. de Thér.*, 29 Fevr., 1872.)

* See a full review of Köhler's able essay on this subject, by Dr. B. W. Richardson, *For. Med. Chir. Rev.*, July, 1872, p. 262.

9. *Alcohol*—(a) *Elimination of*.—A notice of Dr. Dupré's important observation, that a volatile substance is found in the urine after six weeks of total abstinence, and even in that of a teetotaler, which gives the reactions ordinarily employed for the detection of small quantities of alcohol, was furnished in the *Report* for August, 1872. From some previous experiments the author concludes that—
 1°. The amount of alcohol eliminated *per diem* does not increase with the continuance of the alcohol diet; therefore, all the alcohol consumed daily must, of necessity, be disposed of each day, and, as it is certainly not eliminated within that time, it must be destroyed in the system. 2°. The elimination of alcohol following the taking of a dose or doses of alcohol is completed within 24 hours after the last dose of alcohol has been taken. 3°. The amount of alcohol eliminated in the breath and in the urine, is a minute fraction only of the amount of alcohol taken. Alcohol is eliminated through the kidneys, lungs, skin, and bowels, but chiefly by the two former.—(*Practitioner*, March, 1872.)

(3) *Effect on Work*.—Dr. Parkes, continuing his labours, has made some researches on this point, in which the conditions of the experiments were rendered carefully exact. The diet of a man was so arranged that he received every day precisely 20 grammes of nitrogen. For six days he remained as nearly as possible at rest. Then for three days he worked as hard as possible. Then followed three days more of rest, and then another three days of hard work, but this time he took within the day 12 ounces of brandy = 5·4 ounces of absolute alcohol. Throughout these five periods the food was constant (oatmeal, milk, salt, and water), and the urinary and alvine evacuations were carefully collected, weighed, and analysed. The average daily excretion of nitrogen in grammes was:—

	Urine	Fæces	Total
I. Rest, - - -	15·183	3·765	18·948
II. Exercise, on water, -	16·274	4·981	21·255
III. Rest, - - -	15·850	3·251	19·101
IV. Exercise, on brandy, -	15·750	4·372	20·122
V. Rest, - - -	15·007	3·215	18·212

The brandy did not affect either of the excretions; it increased the heart's action, that was all; and it materially hindered the man's work.—(*Lancet*, September 28, 1872.)

10. *Camphor—its physiological action*.—Dr. J. Harley put out, in an interesting paper, in the *Practitioner* for October, 1

the ignorance which prevails as to the real medicinal properties of camphor, which is chiefly due to the insignificant doses in which it is commonly exhibited—*e.g.*, one fluid ounce of aq. camphor = about $\frac{1}{2}$ gr. of camphor, whereas 5 grs. are insufficient to produce any appreciable symptoms. After quoting several cases of poisoning by camphor, and giving in detail an account of two cases (dysmenorrhœa, and frequent seminal emissions), in which camphor was freely employed by himself; he shows that the physiological effects were uniform, and that in doses not exceeding 35 grs. there were simply giddiness, languor, and a diffused feel of warmth through the body—the digestive, and circulatory with slight exceptions, and respiratory apparatus remaining unaffected. The solution employed was, camphor, 3iv.; water, 3ij.; rectified spirit, to 3vi. 3i. = gr. v. camphor. The dose was taken in one or two tumblerfuls of warm water. The urine was always normal in quantity and composition, and was entirely free from the odour of camphor, which, moreover, was not in the least degree appreciable in the cutaneous or pulmonary exhalations of either patient. He concludes with an account of a few experiments on moths and the mouse, and sums up by saying that camphor appears to exert its action chiefly upon the cerebral lobes, causing at first depression of mental power, giddiness, and somnolency. Delirium comes on later, and if the use of the drug be continued, it produces great depression of muscular power and intellectual lethargy. It certainly is not a vascular depressant in medicinal doses, and seems both to be rapidly and completely absorbed, and as completely decomposed.

11. *Conia*.—Dr. J. Wilkie Burman has made a number of experiments on the physiological action of this alkaloid on the lower animals, and on man, and has employed it subcutaneously, chiefly in cases of acute mania. Conia is too irritant to admit of its injection in the free state, and the formula recommended by the author, is as follows:—*R.* Conia, 3ij., m. 12; Ac. Acet. fort., 3ij., m. 40; Spir. rect., 3i.; Aqua destill., ad. 3ij. 5 minims of this solution = 1 m. of conia. Thus administered, conia may be used therapeutically, in doses of from m. ss., to m. iij., in cases of mania, with the result of subduing motor excitement, preventing exhaustion, and promoting recovery. Conia, in this form, does not disturb the digestive functions, interfere with the circulation, nor cause any considerable local irritation.

Specimens of conia, as obtained from different sources, vary

considerably in appearance and strength, and great care should be observed in the use of a new specimen, until its strength is ascertained.

Half a minim of best conia (value, $\frac{1}{4}$ d.) subcutaneously injected, is equivalent in action to about one fluid ounce of the best succus conia (value, 2d.) administered by the mouth.—(Reprint from the *West Riding Lunatic Asylum Medical Reports*, Vol. II.)

12. *Iodides of Potassium and of Ammonium*—*Purpura produced by—and not by Iodide of Sodium*.—The occurrence of purpura and of pemphigus uniformly following the use of iodide of potassium has been observed in a few rare cases, and examples were quoted in the *Reports* for August, 1871, and February, 1872. Dr. Sydney Ringer records another in the *Practitioner* for March, 1872. A lad, aged seventeen, convalescent from acute rheumatism, was ordered 10 grs. of iodide of potassium thrice daily, and on three several occasions a petechial rash appeared on the front and sides of his legs and ankles. The spots disappeared each time a few days after the discontinuance of the medicine. A few days subsequently, 10 grs. of iodide of ammonium were given, and in six and a-half hours after, but two doses, petechiæ broke out on both legs. The spots disappeared in five days. A mixture of 10 grs. of iodide of sodium and 10 grs. of iodide of ammonium, accidentally given, caused a similar rash. When the spots had disappeared, iodide of sodium, in 10 gr. doses, three times a day, was commenced, and was continued for four days without the appearance of any petechiæ or of coryza. A subsequent trial with iodide of ammonium produced spots on the legs in two hours.

13. *Further Observations on Therapeutics*.—Dr. James Ross, who has already written several suggestive papers on therapeutics, contributed some further reflections to the *Practitioner* for January, 1872, which are worthy of attention. When it is maintained that the study of therapeutics must be begun anew, this may be taken in either of two senses. It may be meant that we must reject all the labours of our predecessors, their facts, their classifications, and their interpretation of the facts, or that while accepting the facts they can give us, we must interpret these facts afresh and reduce them to an arrangement thoroughly independent of any thing which has hitherto been done. The first position is indefensible, the second expresses a truth which is felt by the best members in the profession. Therapeutics can be founded upon a scientific

basis, but those who suppose it can be made an exact science, overlook, in the first place, the distinction between a concrete and an abstract science, and, in the second place, between a theoretical and a practical science, *i.e.*, between a science and an art. No one need expect to make therapeutics more exact than the least exact of the sciences upon which it is founded, *e.g.*, physiology. A science is an investigation into the course of nature, but an art proposes to itself an end which it is desirable to attain, and, in the case of medicine, a knowledge of many sciences is necessary for the skilful application of means to the end, *viz.*:—the attainment of health. So far as practical medicine is concerned, what we want is, not the discovery and enunciation of laws of nature, but the enunciation of principles which will enable us to converge a wide scientific knowledge for the achievement of a certain end. We want to fill up the gap which separates the highest generalities of our knowledge and the minutiae of our practice, and he is the most successful practitioner who, with a knowledge of the principles of his art, and of the sciences upon which it is founded, has the faculty of attending to the most minute and multiplied details. With regard to scepticism we ought to remember that for the practical emergencies of life we must act upon evidence much inferior to anything which meets us in the least certain of the sciences. If it were always borne in mind that therapeutics is an art, we should not hear so much of therapeutic experiments. When our object is, primarily, not the ascertainment of truth, but the cure or relief of disease, an experiment in any true sense of the word is impossible. It is well if the result obtained be sufficient to serve as a verification of our argumentation from the theorems of science. Simplicity in prescribing is, doubtless, most desirable, but since all the productions of art are attained by a combination of means, it may be often advantageous in treatment to combine two or more drugs, provided that care be taken that all are concurring to the same end, and that the action of the one drug does not interfere injuriously with that of the other.

14. *Chloral v. Strychnia*.—The supposed antagonism of strychnia to chloral, first broached by Liebreich, and confirmed by Hughes Bennett, has been shown by other experimenters to be not absolutely true. And M. Oré has made some new experiments on rabbits touching this question, which are in opposition to those of Liebreich. His conclusions are:—

1°. 4 grammes of chloral injected subcutaneously, are a *lethal dose* for rabbits weighing 2 kgms.; and 2 grammes for rabbits weighing less than 1 kgm.

2°. If, when the effects produced by this dose of chloral are distinctly manifested, we seek to arrest them by injecting 1·5 mgrms. to 2 mgrms. of strychnia (the first of these two quantities not being fatal to a rabbit weighing 2 kgms., which the second is), the animals succumb in either case.

3°. Throughout the experiment, from the moment of the introduction of the strychnia up to death, *no phenomenon reveals its presence*. Cadaveric rigidity, together with the precipitates furnished by the urine, and by decoctions of the liver and spleen, with biniodide of potassium and decoction of galls, are the only circumstances which prove the absorption of the alkaloid.

4°. From still later experiments, M. Oré finds that not only does the action of strychnia not neutralize that of chloral, but on the contrary, it actually hastens the final result—the death of the animal. Strychnia, therefore, is not an antidote to chloral.—(*Journ. de Pharm. et de Chim.*, Oct., 1872.)

15. *Absorption of Medicines through the Skin*.—The discrepancy met with in the statements regarding the power of absorption by the skin and mucous membranes towards medicinal substances is somewhat singular, and appears to be chiefly due to the different conditions under which experimenters have worked. The affirmative experiments of Dr. Chrzonzewski on this point were quoted in the *Report* for February, 1872; and the conclusions drawn by M. Bremond from his experiments with iodide of potassium are as follows:—1°. The cutaneous absorption of a non-volatile medicine is undeniably established by experiments with medicated vapour-baths. 2°. In ordinary cases, it is possible only at a temperature of 38° C., *i.e.*, one degree, at least, above that of the body. 3°. By the previous use of a vapour-bath, aided by soaping and diligent friction, the skin will absorb iodide of potassium at temperatures of 34° to 36° C. 4°. In the author's apparatus, the cutaneous absorption augments in direct ratio to the temperature of the vapour-bath, to its duration, and to the quantity of iodide of potassium. The salt is in suspension in the watery vapour, and is carried mechanically into the wooden cage of the apparatus. 5°. The elimination of this salt commences about two hours after the bath, increases up to the next meal, after which it seems to diminish, owing to the water

ingested, then becomes more abundant, and ceases completely in twenty-four hours, whatever be the dose of the salt, the temperature or the duration of the bath. 6°. When the patient has taken ten or twelve baths, elimination goes on for three or four days. 7°. The potassic iodide is certainly absorbed as such, for analysis of the aqueous vapour which escapes never reveals a trace of free iodine.—(*Journ. de Pharm. et de Chim.*, Nov. 1872.)

In the same journal is a paper by MM. Jamin and de Laurés, who spent a season at the Nérès waters, and repeated the old experiments of Sanctorius, on the variations of weight of the human body when plunged into water. Their observations confirm the conclusions of M. Durrieu, who has enunciated a law which reconciles the perplexing statements made by different experimenters, viz.:—Every individual preserves an invariable weight in a bath of moderate temperature—what he terms an *isothermal bath*; he absorbs and gains weight if the temperature be lowered; he loses, on the contrary, if it be raised, and this loss increases very rapidly when the temperature of the water rises from 36° to 48° C. A healthy man, it should be remembered, absorbs about 4,000 grm. of nourishment daily, expels 1,500 grm. of residuum, and consequently, assimilates 2,500 grm. of material, which disappear in 24 hours, by the lungs or by the skin.

SPECIAL THERAPEUTICS.

16. *Cyanosis from Nitrate of Silver, removed by Iodide of Potassium.*—Dr. L. P. Yandell mentions two cases occurring in young merchants, both of whom had been unsuccessfully treated for epilepsy in their youth, by nitrate of silver, and were disfigured by the staining of their skin. Both contracted syphilis, and, for tertiary symptoms, got iodide of potassium in 10–60 gr. doses, thrice daily for some months, in connexion with ferruginous or bitter tonics. One of the patients was forced to discontinue the iodide on account of its disagreeable effects upon the system. The other took it till all traces of syphilis had passed away. In each case the fading of the stains was gradual. In the one case, there is a faint, scarcely perceptible, trace of discolouration remaining; in the other, which was much the darker of the two, there is not a shadow of the disfigurement. The iodide was not given in either case with reference to the *argyria*, and its beneficial effects were observed by Dr. Yandell accidentally more than a year after their occurrence. It may be

well to state that both patients were treated by the moist mercurial vapour-bath, during much of the time that they were using the iodide, and the abundant diaphoresis may have assisted the action of the potassic salt.—(*Amer. Practitioner*, June, 1872.)

17. *Ergotine—its use subcutaneously in Hæmorrhage.*—Dr. Drasche (*Wien. Med. Wochenschr.*, Nos. 37–40, 1872) believes that ergotine must be regarded as the best of all hæmostatic agents for hypodermic use. On two persons, in robust health, the injection of from $\frac{1}{2}$ gr. to 2 grs. was uniformly followed by a diminution in the pulse of from 4 to 6 beats, and the sphygmograph showed a simultaneous contraction of the calibre of the vessels. The temperature was slightly raised, respiration normal, no influence upon the urine, and the health was quite undisturbed. Some local irritation was caused, but usually passed away soon. Dr. Drasche has employed this method chiefly in tubercular hæmoptysis in doses of $\frac{1}{2}$ to $1\frac{1}{2}$ grs., and in most of the cases the efficacy of the ergotine was very prompt, when other hæmostatics had previously been tried without success; in others, although less speedy, it was still successful, and in one case only out of nine did it fail. In two cases of epistaxis the effect was very speedy, and the dose required very small. It has also been used with marked advantage in hæmatemesis, intestinal hæmorrhage, and hæmorrhagic scorbutus. The best vehicle is glycerine, containing 5 grs. in 3i., and the injection is best made in the region of the pectoral muscles. When the injections have been long continued, or have been too strong, tingling of the finger-ends and cramps of the hands have been sometimes complained of.—(*Med. Times and Gazette*, Nov. 16, 1872.)

18. *Propylamine in Rheumatism.*—Attention was called to the use of this volatile organic base by Awenarius, of St. Petersburg, nearly twenty years since, in the treatment of acute and chronic rheumatism, in which he had prescribed it in more than 250 cases. An abstract of Dr. Namias' not very conclusive paper was given by Dr. Duffey in the December number of this Journal, and the experience of Dr. Gaston, of Indiana, for the past eight years, is in accord with that of the Russian professor. He is so certain of the value of this drug that, having diagnosed acute rheumatism, he promises relief in thirty-six or forty-eight hours. From two to eight drops of propylamine in a spoonful of water, every two

hours at first, are prescribed, and speedily assuage the arthritic pain and tenderness.—(*Rev. de Thér. Med. Chir.*, Juin, 1872.)

19. *Sarsaparilla in Syphilis*.—Dr. C. Allbutt maintains the value of this old and much-abused remedy in the tertiary and visceral forms of syphilis of long standing, especially as suited to fill the gap when mercury and iodide of potassium have failed. He administers from ℥iv. to ℥x. of the decoction three times a day, i. e., about a pint or a pint and a half in the twenty-four hours—(*Practitioner*.)

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Wednesday, December 11, 1872.

HENRY FAMES, M.D., Honorary Secretary.

DR. HAYDEN, in the Chair.

On the Use of the Direct and Induced Currents of Electricity.—No. III.*

By WALTER G. SMITH, M.D., Dubl.; Fellow and late Examiner in Materia Medica, K. & Q.C.P.I.; Assistant Physician to the Adelaide Hospital.

AFTER many vicissitudes of favour and neglect it would seem that electricity has finally struggled through the period of transition, and has gained for itself a sure footing among the resources of the physician and surgeon. It is now generally conceded that electricity is a valuable remedy, which we cannot afford to despise, and its cultivation is carried on with perhaps as much enthusiasm, and certainly with better directed aims, than at any previous era of its history. This marked change is, no doubt, mainly due to the pregnant discoveries which have been made within the last forty years in the physics of electricity; witness the unfolding of magneto-electricity and of electro-magnetism by Faraday, and to the improvements in the mechanical details of the necessary apparatus; but, in addition, the recognition of this power as an agent of real utility by the leaders of the profession, its rapid and increasing development in clinical use, and the due value assigned to it in some of the best modern works, have contributed no small share to this result.

Among the more recent and positive gains to electro-therapeutics we can fairly reckon the better discrimination as to the respective functions of the induced (so-called *interrupted current*), and of the direct (or so-called *constant or continuous current*), and the indispensableness of the latter in

* Nos. I. and II. Dublin Quarterly Journal, August, 1869; August, 1870.

certain cases of paralysis; the use of electricity in laryngeal affections, and the more certain relief and cure of severe forms of neuralgia and of some conditions of perverted sensibility; the wider diffusion of a correct knowledge of the methods of electrization; the application of electricity to diagnosis and prognosis, a subject most unjustly ignored in ordinary practice; the proved possibility of directly galvanizing the brain and spinal cord; and, lastly, the utilization of electricity in surgery for its heating powers, or for its chemico-vital effects in causing coagulation of the blood, and in producing discussion of tumours.

Any reference to "general faradization" and the so-termed "galvanization of the sympathetic" is designedly omitted, for these methods do not appear to me to rest as yet on a sufficiently sound foundation. Niemeyer, whose opinion is justly entitled to respect, and who certainly cannot be charged with credulousness, was firmly convinced "that the introduction of the constant current into practice is one of the most valuable advances of modern times, and that *in the constant current* we have a means more powerful than any other of modifying the nutritive conditions of parts that are deeply situated." And it seems to me, that this vital influence of electricity on nutrition has scarcely obtained the attention which it merits, and that in our practice and teaching too exclusive a part is ascribed to the merely stimulant action of the current on nervo-muscular tissue. Electricity is a muscular stimulant, but it is something more.

On several previous occasions I have ventured to bring before this Society some practical observations on the uses of electricity in paralytic and neuralgic cases, and I purpose now to select from a record of about fifty clinical histories a few illustrations of the good which can be effected even in unpromising cases, and to point out those instances in which electricity proved powerless for good from the nature of the case or from other circumstances.

Now, at the outset, in regard to the unsuccessful cases, it must be observed that several considerations tend inevitably to swell the numbers of this class. For example, any one who interests himself specially in this branch of therapeutics is often requested by other medical men to try the effects of electricity, as a forlorn hope in unpromising cases, perhaps with existing and irremediable organic disease in an advanced stage, and so under the most disadvantageous conditions. Again, patients who suffer from obstinate or incurable affections, especially when of an anomalous character, will frequently seek after, nay, insist upon electric treatment, clinging hopefully to it as a last resource, and are naturally reluctant to abandon all prospect of recovery, while in not a few instances the physician himself is strictly justified, even with but a slender warranty of success, in carefully, though empirically, testing the effects of electricity on various diseased conditions, especially when we remember the

incompleteness of our pathological knowledge and the uncertainty which still hangs round neuro-physiology. Careful clinical observation is fully entitled to thread its way where as yet the light of exact knowledge has not penetrated, and in not a few cases which are rightly considered *cures* success rewards our efforts when explanation is completely at fault. "Certainly, the action of electricity as a therapeutic agent is in some cases exceptional and most satisfactory."^a

Of spasmodic affections I have had but a few under my care, and in these my experience of electricity, whether galvanic or faradic, and no matter how applied, has so far been unfavourable, either in shaking palsy, writer's cramp, or in localized spasm of the cervical or other muscles. It is certainly singular how obstinately a small, and in itself an insignificant spasm, when once firmly established, will so often refuse to yield to any anti-spasmodic treatment, or to be coaxed into submission by any sedative means.

In advanced cases of progressive muscular atrophy, in chronic general spinal paralysis with marked atrophy of the muscles, in old spinal paraplegia, in aggravated lead palsy with loss of re-action to either form of electricity, and in not a few cases of infantile paralysis, our best efforts must often be doomed to failure, beyond preventing any increase of the atrophy or deformity, and it is both useless and sometimes mischievous to persist too long with electric treatment.

The chemical action of the electric current I have employed chiefly in *nævus*, and in some instances, with remarkably rapid and successful results. In fact, for small superficial *nævi* and vascular growths where it is desirable to leave no mark we have no agent so ready and efficacious as electrolysis, but this point I shall reserve for another opportunity, and will pass on now to the consideration of paralytic cases.

Those I will mention are grouped under the following heads:—
1°. Bell's palsy; 2°. Traumatic paralysis, due to pressure or other local injury; 3°. Paralysis from lead. In the first two classes the muscles are cut off from the influence of the spinal cord, constituting the "spinal paralysis" of Marshall Hall.

I. BELL'S PARALYSIS.

CASE I.—Mr. O'F., a medical student, aged twenty, applied to me in May, 1870, for right facial paralysis contracted four days previously after exposure to cold air. Next morning he found that he could not

^a I need scarcely remark that electricity is concerned rather with the relief of the effects of disease than of the cause, and that, as a general rule, the cause must first be removed or have ceased to act, before we can expect good to accrue from its use. Neither does its employment interfere in the least with the action of any other medicinal remedies which the nature of the case may require.

spit straight, and the usual signs *gradually* developed, although without much oral distortion, and at first there was numbness along the right side of the tongue; hearing was more acute on the paralysed side, and he could not retract the right ear. As to treatment, it is sufficient to state that both the direct and the induced currents were used, that the muscles rapidly regained their functions after seven or eight applications, but for some time afterwards the tears had a tendency to escape from the eye, and he could not whistle so perfectly as before the accident. In the commencement twitchings occurred in several of the muscles, especially the orbicularis oculi. In the course of treatment I observed a circumstance which was new to me at the time and which is, at least, quite the exception. It is commonly stated that, at any rate after a few days, the muscular contractility to induced electricity is below par on the paralysed side, or may even be abolished altogether, and that this condition will even enable us to diagnose an eccentric facial palsy from one of central origin, in which the electric contractility is preserved intact. As a nearly universal rule this is, no doubt, true; but in this case the paralysed muscles uniformly responded to a weak induced current, and also to a moderate direct galvanic current *more actively than the healthy* muscles for about a week, after which contractility became normal.

CASE II.—In December, 1870, Miss P—— called on me, by Dr. Stokes' advice, for incomplete right facial paralysis contracted five weeks previously. Even on the first trial she could nearly close the eye under the influence of a gentle primary induced current, but both galvanic and faradic excitability were decidedly diminished, and the current was less felt on the paralysed side. Yet after a short time (three weeks) the galvanic excitability was heightened, for six (Leclanché) cells caused vigorous contractions on the paralysed side and none on the left side, and, what is more remarkable, the current from a *single cell* evoked unmistakable contractions of the paralysed half of the sphincter oris. The progress of this case was unusually tedious. Excitability to the induced current was very slowly regained, and even when motility was nearly perfect and the facial distortion had disappeared, the paralysed muscles responded more to a direct galvanic current and less to the induced current than the healthy muscles. Rapid interruptions of the direct current by the hand caused *tonic* contractions of the muscles. The treatment extended over about two and a half months, and perhaps some reason might be found for this in the fact that the paralysis had set in with pain, had existed for more than a month before treatment was commenced, and occurred in a lady of a highly nervous and excitable temperament.

CASE III.—About the same time a young gentleman, aged fifteen, from

Kilkenny, was also sent to me by Dr. Stokes for left facial paralysis which had developed six weeks previously without any apparent cause. He remained a month at school without any local treatment, and partly regained power over the muscles. The muscles reacted feebly and slowly to a weak induced current, and more readily to a galvanic current than the healthy muscles; and, as in Miss P——'s case, even the feeble tension of a *single cell* sufficed to excite the oral muscles, and a rapid hand-interruption of the direct current caused *tonic* contractions. Again, even when motility had returned the excitability to the induced current was still below par. The duration of treatment was one month, and the progress towards recovery, though gradual, was steady.

CASE IV.—A man, aged thirty-nine, was admitted into the Adelaide Hospital under Dr. Head, in April, 1870, suffering from right facial paralysis originating six weeks previously after working in the cold air. No treatment had been adopted before admission. Contractility to the induced current was nearly if not altogether abolished, but a 30-element Pulvermacher-chain elicited decided contractions in the paralysed side only. Faradization and galvanization were employed for more than two months, when the distortion had nearly vanished, but the faradic excitability of the muscles remained much diminished. The duration of the case before treatment, and the marked disturbance of the electric excitability pointed from the first to a tedious recovery.

CASE V.—A little girl (A. P.), aged twelve, applied at the Adelaide Hospital, September, 1870, for right facial paralysis which appeared three weeks previously, after sleeping in a draught of air. Nothing was done for her for some days; the paralysis was but partial, and, on examination, a feeble galvanic current affected the paralysed muscles much more than the sound ones. She at first improved rapidly under the electric treatment, which was continued for two weeks, then more slowly, but, ultimately, all traces of the paralysis faded away, and, as in the other instances, muscular excitability remained diminished even when motility was nearly restored.

Summary.—These cases, coupled with the six recorded in my previous paper (*Dublin Quarterly Journal*, August, 1869), including in the eleven cases seven on the right side and four on the left, show—

1°. That in the large majority of cases contractility to faradization was much reduced, but that in one case (Case I.) it was actually *heightened*.

2°. The re-action of the paralysed muscles to the direct galvanic current is often exalted.

3°. A direct current so feeble as that derived from a single (Leclanché)

cell may be competent to excite the paralysed muscles ; at least, the orbicularis oris (Cases II. and III.)

4°. Ulceration of the cornea can undoubtedly arise as a consequence of exposure of the globe of the eye, from patency of the eye-lids (Case VI., Series 1).

5°. Permanent tonic *contraction* of the paralysed muscles may ultimately ensue, and so simulate paralysis of the *opposite* side (Case V., Series 1).

6°. Although in many cases the use of the direct galvanic current is requisite, it cannot be too much insisted upon that, as an invariable rule, in applying electrodes to the head and face, feeble currents should be used, and that for a short time only.

I have often seen giddiness, faintness, pain in the head, and sometimes drowsiness produced by a current of very moderate strength (say five or six cells) applied across the temples, or between the mastoid process and the forehead ; again, nausea, vomiting, and even convulsions may be provoked, and permanent blindness has been inflicted by too powerful a current.

II. TRAUMATIC PARALYSIS.

Of the four cases to be presently related, two were due to compression of the nerve during sleep, and two to a more serious injury of the affected nerve.

CASE VI.—A smith (G. N.), aged forty-eight, applied to me in September, 1870, on account of weakness in the right arm. A fortnight previously he fell asleep while drunk, and on awaking found that the right forearm was powerless, and that he had complete drop-wrist. The muscles only partially regained their function in the absence of treatment, but after three or four applications of magneto-electricity he recovered their use, and it was observed that the electro-contractility was but little, if at all, impaired.

CASE VII.—Last May a gentleman (W. B.) was recommended to me by Dr. Stokes, to have electricity applied for paralysis of the right arm. Four days previously he experienced on awakening a sense of tingling in the back of the right arm, and was unable to extend the wrist or fingers.

Muscular irritability was not sensibly diminished to induced electricity, and he recovered the use of the muscles after six sittings.

Both of these cases show that nerve injuries, even when they have continued for some days, do not necessarily bring about any marked diminution in the electro-muscular contractility.

Although in civil practice we rarely have an opportunity of studying the effects of gun-shot injuries of nerves, two examples of this class of

accident have come under my notice, one of which, that of a farmer shot in the arm, has been already published (*Dublin Quarterly Journal*, August, 1870).

CASE VIII.—In the course of last September an Austrian gentleman called on me with an introduction from Dr. Althaus, of London, asking me to continue the electric treatment which he had initiated. About seven weeks previously he was wounded by a bullet in the left upper arm. The ball entered about the middle of the arm, close to the inner edge of the biceps, and emerged at the opposite side. Much blood was lost at the time, and he suffered severe consecutive pain, which was relieved by morphia hypodermically. The median nerve was evidently injured, for the flexors of the thumb and forefinger were completely paralysed, and there was anæsthesia of the thumb, forefinger, and outer half of middle finger. When he came under my care the left forearm was flabby, and somewhat smaller than its fellow; the thenar eminence soft, the abductor indicis much wasted, and the circulation of the hand very defective. He could not flex the index finger, nor the last joint of the thumb, and the palmar surface of the index finger was completely anæsthetic to touch or to electricity. In this case induced electricity failed to stimulate the palsied muscles to contract, but a galvanic current from fourteen (Leclanché) cells caused distinct contraction of the flexor pollicis longus, and after a few days even nine or ten cells. He was acutely sensitive to the burning feel of the galvanic stream; the application of the faradic wire-brush to the deadened fingers rapidly improved the sensation; and he partly regained the power of flexing the forefinger. He was obliged to leave town soon, after but a fortnight's treatment, far too short a time in such a serious case, yet with a good prospect of ultimately complete recovery, for the slumbering motility had been aroused, and I believe that, in not a few cases, satisfactory progress towards health will go on steadily, if once the palsied parts be roused from their inaction by appropriate means, for the nervo-muscular apparatus of a limb may, so to speak, grow sluggish, and fail to respond to the motions of the will long after all anatomical effects of the injury have vanished, while their structural continuity is really restored, and they are competent to fulfil their proper functions.

CASE IX.—Towards the close of last year a gentleman came under my care for a localized paralysis of the leg. The preceding April he had received a hurt on the outside of the right knee from slipping on a stone step. In a few days he lost the power over the anterior muscles of the leg, and since that time has been unable to flex the foot.

When I saw him the right calf was one inch less than the left, and every movement was perfect, except that of the peroneal and anterior

tibial muscles, whose motility was quite extinguished, and the only effect of a pretty strong induced current applied to them was to call into action the antagonistic muscles, while a 120-element Pulvermacher's chain also failed to elicit any sign of contraction. After ten days' treatment a *slowly* interrupted chain-current caused contraction of the extensor pollicis proprius and the descending current acted more forcibly than the ascending. The treatment was steadily pursued for a month, although with but slight benefit, when a Leclanché battery was procured, thirty cells of which produced strong contractions, and eighteen had some effect. Galvanization and faradization were regularly employed for another six weeks; eleven cells then sufficed to produce some muscular contraction, but still the induced current remained without effect, except on one occasion. Treatment was now suspended for some months, as he could walk without fatigue, and had partly regained the use of most of the paralysed muscles, and in October last, *i. e.*, eleven months after his first visit and eighteen months after the receipt of the injury, he could flex the foot, turn it outwards, walk well, but was still unable to extend the great toe. He now wished to see if the cure could be completed, and after the second sitting (a twenty-cell current applied) he was *able to extend the toe himself* for the first time, and after a few applications, completely regained voluntary power over the muscles. The leg had increased in size, and the faradic excitability had nearly returned.

The noticeable points in this case are:—The completeness of the motor paralysis without any anæsthesia, the total extinction of muscular irritability to ordinary stimuli, the unpromising aspect of the case from its duration, the nature and the age of the patient, and the beneficial results attained by perseverance, although scanty encouragement was had for some time, while the restorative powers of nature certainly showed little tendency to repair the mischief that had been done.

In speaking clinically of the diminution or abolition of electro-muscular contractility after nerve-injuries, these terms must be taken with some reservation, and it is to be borne in mind that this faculty is, as a rule, tested only through the moistened skin and by currents of moderate strength. Hence, we should not be misled by the apparent discrepancy between the results so obtained, and those obtained by physiologists who have explored the excitability of nerves and muscles when *laid bare*. In the latter case, for example, it has been ascertained that, in the lower animals, irritability of the muscles is preserved many months after section or removal of a portion of the facial nerve, and that, in fact, contractility persists so long as the proper muscular tissue has not disappeared. M. Vulpian has quite recently made some further interesting experiments (*Arch. de Physiol. Mars. Mai. Oct.*,) to confirm and extend what he had already established some years previously, *viz.*, that, in respect to induced currents, there is no real

opposition between what is observed in man and in the lower animals. Although the farado-muscular excitability is undoubtedly always diminished shortly after a severe nerve-injury, yet the muscles, even when excited through the moistened skin, usually preserve for months some degree of their faradic contractility, at least to a *powerful current*, and when the muscles are exposed and the electrodes applied directly to their surface, there is no doubt at all that contractility lingers indefinitely, so long as the animals remained under observation, *i.e.*, for three or four months. Vulpian also dissents from the opinion held by Erb and others that the susceptibility of the muscles to the action of a direct galvanic current is exaggerated up to a certain point, while, at the same time, the action of induced currents is enfeebled; for, on the contrary, he obtained variable results, the most usual being *diminished action* of the galvanic current on the *paralysed* muscles as compared with the healthy side. These experiments were made on the facial and sciatic nerves of rabbits, dogs, and guinea-pigs, and the results obtained were independent of the kind of lesion to which the nerve was subjected.

III. LEAD PALSY.

On this affection I shall make but one or two remarks, without detailing any cases. In two instances within the past year, I have seen more or less paralysis of the legs as well as of the arms, and one of the patients walked at first with a staggering gait, not unlike that of "ataxy." The gravity of the paralysis certainly seems to bear no constant relation to the degree of muscular atrophy; for it will, I think, be found that every grade of paralysis may occur with or without much atrophy of the muscles, and cures will undoubtedly be effected even when the contractility of the muscles to induced electricity is apparently quite lost. In severe cases, although the patient may be so far relieved that he is able to return to his work, the palsied muscles seldom regain their full bulk and vigour. Flabbiness and want of consistence of the muscles is a simpler and probably nearly as good a test of their vital condition as electric contractility, and the sensibility of the skin and muscles, so far from being always impaired, seems often to be exalted, and muscular faradization by a moderate current is frequently attended with a deep-seated acute pain.

As clinical facts I may mention that it is singular that the supinator longus is not usually, so far as I have seen, completely paralysed, nor does it lose its electric excitability readily, and that a rapidly interrupted direct galvanic current sometimes has no effect, when a slowly interrupted one has. Again, I am convinced, from repeated observation, that the statement commonly made in the text-books that the extensors only of the wrists are engaged, and that the corresponding flexors escape entirely, requires some modification, *e. g.*, "lead poisoning always affects the upper

extremities first, attacking the extensors of the fingers, hands, and arms, in succession, while the *flexors remain quite free from disease.*" (Niemeyer, II., p. 326, 1870.)

On the contrary, it is sufficient to test the grasping power of the hand, to feel the flabby condition of these muscles, and to observe their atrophied state and the diminution of electro-contractility which so frequently obtains, to be convinced that the dictum, that the flexors escape the poison of lead, is only relatively true.

The CHAIRMAN said he had not had a great deal of experience in electric therapeutics, but he had seen sufficient to enable him to confirm the statements of Dr. Smith, especially with regard to the curability of painters' paralysis. He was sure it was an error to assume that the flexor muscles were not involved in such cases. He had a case of that kind in hospital, and after some months the patient got quite well—the muscles had recovered volume and sensibility. There were several interesting questions raised in the paper, especially as to ulceration of the cornea. No doubt, that might arise from exposure of the eye, but on the other hand, where the eye had not undergone any exposure, it might arise from paralysis, and in that case, the fifth pair of nerves was generally involved.

DR. LYONS congratulated the Society on a paper of such originality and merit having been read at so early a period of the Session. An interesting case came under his observation some three or four years ago, in which he had employed the electric current with advantage. It was an instance of a rather unusual form of double Bell's paralysis. It occurred in the person of a man who, after having drunk to excess, slept in a damp cellar; and when he awoke in the morning, his face was paralysed on both sides. He came to hospital next morning, and not having before seen a case of double Bell's paralysis, he (Dr. Lyons) looked upon it at first as a case of perfectly hopeless central paralysis. However, having examined the man carefully, he took him into hospital; had him under observation for some time, and tried various methods of treatment. Nothing was so effectual as electroism, and the case ended in complete recovery. He had tried it in various other forms of paralysis, and could bear out Dr. Smith's views as to its efficacy in cases of chronic painters' paralysis, where the ordinary forms of treatment were resisted. No doubt, it might be useful in the first instance, in an early stage of the disease, but it was especially in those chronic cases of a recurrence of the malady in painters of broken down constitution, where other plans had been tried and failed, that electroism would be found useful. As to the continuous current, his attention was called especially to it at the time the British Medical Association visited Dublin, when Dr. Althaus did him the honour of becoming his guest. Prior to that he had the continuous current in

operation in several cases in the Whitworth Hospital, and he had the advantage of Dr. Althaus' advice in a case where they could not hope for a successful result. It was a case of benign osteo-sarcoma, occurring in the leg, and the patient experiencing but little suffering, would not submit to an operation, although, they were convinced that it would be ultimately necessary.

In that case the continuous current was applied for a very considerable period indeed, and with the effect of making a certain impression on those parts of the large tumour, on which it could be expected to exert any influence.

There was another class of cases, a very singular class, in which there was a localized paralysis, sometimes of one, sometimes of the other muscles within the orbit—a class of a very interesting character, and involving great difficulty of diagnosis in the first instance. They all knew what an important point in cases of centric lesion double vision was—it was often the result of deep-seated cerebral malady, which usually proved rapidly fatal. On the other hand there was a class of cases where double vision was due to a local paralysis of the external or internal rectus. One very interesting case of this nature had come under his observation. It was that of a gentleman somewhat advanced in years, and whose history was that of very close application to study, prolonged to a late hour in the night, and in whom there was some headache. There was some reason to think it was an instance of deep-seated cerebral disease. After very careful analysis of all the symptoms, Dr. L. arrived at the conclusion, that it was paralysis of the external rectus of one eye. The use of the electric current, gradually applied twice a week, with other remedies, had the effect of ultimately restoring vision perfectly, and the diagnosis was confirmed. Double vision was one of those symptoms that usually occurred in the early period of true cerebral disease, and the diagnosis was often very difficult in the earlier stages. On the other hand, it was sometimes due to a local lesion, as in the case he had just mentioned. Another very remarkable instance was due to those diseases that were ascribed—he would not say accurately, for he did not agree with the theory—to syphilitic lesion.

He called them cases of concentric paralysis, when there was strabismus, paralysis of the muscles of the face, and loss of sensibility in limited parts of the face. He remembered a case which occurred in his clinique, in the Whitworth Hospital. It looked like centric paralysis. Careful analysis, and the use of electricity with success, led him at an early period to the conclusion, that it was a peculiar form of peripheral paralysis—not following out the course of any particular nerve, but operating on particular points of the distribution of nerves in the face. The strabismus was cured, and the anæsthesia on the side of the nose, and the zygomatic muscles were brought again into activity.

DR. HENRY KENNEDY said, that Dr. Smith's paper was one of much practical interest; nor could there be any doubt of the great power of this agent. Like so many other agents it had, till recently, fallen into disuse, though formerly it had been used very generally in almost every disease in which it was now found to be so useful; including even the dispersion of tumours. The only disease he did not recollect it to be tried in was aneurism, and speaking of this subject, he wished to call the attention of the meeting to a very valuable paper, just published, on the treatment of that grave disease, thoracic aneurism; and where the results were quite enough to justify a further trial of electricity. Speaking of paralytic cases, too, he wished to direct attention to a paper published some years since in the *Dublin Journal*, by a London Physician. The cases detailed were, so to speak, of a much severer kind than any given this evening; and yet, by great perseverance in the use of electricity, several of them were cured. He regretted not recollecting the name of the author. He wished to state in conclusion, that the local treatment by electricity was not to exclude general treatment from which good results were often obtained.

DR. EAMES was sure that Dr. Smith did not use electricity by way of experimenting on his patients, but that he employed general remedies in all cases in which they were required. Dr. Smith had not alluded to the treatment of neuralgias, for, on another occasion, he had brought that before the Society, and he had had the most excellent results. He (Dr. Eames) had also met with excellent results in the treatment of neuralgia by the continuous current. He lately had under his charge a man who had received a very severe fall, being thrown on his back. Coma was induced and lasted for some time, and was followed by paralysis. He believed that man's recovery was mainly due to the use of the direct current of electricity, which was used daily for a long time.

DR. HEAD hoped Dr. Smith would take another opportunity of explaining this subject to those members of the Society who had not studied it. There was no one, who kept himself up with the progress of medical knowledge, but must know that electricity was of a certain value in such cases as were suited for it. It never removed the cause of paralysis, but only removed the effects, restoring the sensibility of the parts that had suffered from paralysis. Whatever the cause might be, that cause must be removed by other means besides electricity.

DR. SMITH, in reply, thanked the Chairman and the members who had spoken for the encouragement they had given a junior member to bring forward on any special subject before the Society. He had noticed some of the special affections referred to by Dr. Eames. He had only seen one case of double Bell's paralysis, but he

did not use electricity; and with respect to orbital paralysis, he could only test it in cases of ptosis. He never intended to ignore the employment of other means of cure—at least in such cases as lead palsy, where the poison could be affected in some degree by general treatment; but in cases of local paralysis due to nerve injury, constitutional treatment was clearly useless. He agreed with Dr. Eames as to the efficacy of electricity in anæsthesia, and also as to the mode of operation—the wire brush and the induced current. The point raised by Dr. Head was an important one, and should be borne in mind if they would avoid over-estimating or under-estimating the value of electricity—namely, that electricity was more concerned with removing the effects of disease than abolishing or removing the cause of the disease itself.

On Tapping in Abdominal Dropsy. By ROBERT D. LYONS, M.D., F.K. & Q.C.P.; Physician to the Whitworth and Hardwicke Hospitals; Professor of Practice of Medicine in the Catholic University.

DR. LYONS said his object in bringing the subject of tapping in abdominal dropsy before the Society was to elicit the experience of the practical men he saw around him on one or two points of clinical interest, and not because of anything novel in what he had to lay before them. He would present to them a brief record of seven cases in some of which life had been prolonged for some time, and in others for a very considerable period, by the simple operation of tapping the abdomen. These seven cases occurred in females, who were all living at the same time. Several of them had been tapped a very considerable number of times, and a few points in each case might be worth a brief notice. The first case was that of a female aged forty, the mother of a large family, and her history was, as briefly as possible, that which led up to the establishment of what he diagnosed as an advanced stage of cirrhosis of the liver, with the effusion of a very large quantity of fluid into the cavity of the abdomen. The effusion having reached a very considerable amount, this patient was tapped for the first time during his absence in London in 1870 by his colleague Dr. Gordon, and after his return from London was tapped at repeated intervals varying from three weeks to a month—in all the total number of 35 times. And then arrived this curious condition of things, that the secretion, which amounted in a period of three weeks or a month from 14 to 16 quarts, now appeared to be scanty. She was tapped for the last time on the 10th November 1871, and had since that operation gone on with a certain amount of fluid in the abdomen, but not requiring tapping. Subject to that the patient appeared to have become stationary; and although there could be no doubt there was a considerable amount of fluid in the cavity of the abdomen, the woman was able to go about her work, look at

family, and was, on the whole, in a very tolerable state of health. Here, then, was an instance of a patient tapped, in all 35 times, in a period of now over two years and a-half, and her life had been prolonged certainly for that period, and probably for a considerable time to come, while her condition when first tapped was such as to leave little hope indeed that her life could be prolonged even by the operation of tapping. She had a considerable amount of hæmatemesis. She was exhausted to an extreme degree; and after the operation of tapping and the removal of 16 quarts of fluid she got into a tolerable state of health; and with the renewal of tapping every three or four weeks, she was carried on for the time specified. He did not contrast this case with those extraordinary cases where women had gone on for years, and been tapped 100 times and upwards, but he thought that in a practical point of view it was worth mentioning. The next case was also remarkable in some respects. It was that of a woman who was in hospital at the same time as the first mentioned patient, and in whom the condition of the liver appeared to be that of enlargement with bossy protuberances which could be felt under the parietes. There was a certain amount of cardiac disease, a loud systolic bruit referable to the mitral orifice. This patient was brought into hospital in what he did not hesitate to describe as a moribund condition, and he tapped her rather with the hope of giving her a few hours of comparative ease before expiring from the state of excessive distress she was in, than from any reasonable idea of prolonging life for even a few days. About sixteen quarts of fluid were drawn away in this case also, and the woman then rallied in a remarkable manner, and fully five months elapsed before the abdomen filled again, and it became necessary to tap her. She was in the same ward, and in the bed opposite to the other patient; and it was remarkable to see one patient filling every three weeks, and the other not requiring tapping for five months. At the end of five months he tapped her again with the result of bringing away the same quantity of fluid, and fourteen months now elapsed before it was necessary to tap her again. She had the cardiac disease, the enlarged liver, the bossy protuberances on it plainly perceptible; but she had the most extraordinary determination to live he had almost ever seen manifested in any patient. During the interval he had mentioned, he constantly saw her carrying on her occupation as a fruit seller and earning a livelihood in the streets, struggling to maintain her family. She was admitted to hospital again, at the beginning of the present winter session, and the secretion that took place then was very large, and after the third tapping seemed to accumulate very rapidly and at an average period of four weeks, it having been necessary to tap her every month since September—being tapped in all seven times within a period of two years. This case was interesting in this point of view—that each of the last four tapplings had called into

play a localized peritonitis of the most marked kind, just over the site of the most prominent of the tumours on the liver. A most intense peritoneal friction was audible on the application of the stethoscope, and distinctly sensible to the touch of the physician had been developed in each instance, and in two or three instances with such intense pain, that it became necessary to apply a leech or two over the part on which it subsided. In neither of these cases was there any renal disease. Three other cases presented very enlarged liver, a very considerable amount of jaundice, the urine scanty in amount and loaded with lithates, no cardiac disease, no renal disease, the abdomen extremely distended, great distress, imminence of death and immediate relief and restoration to a comparative condition of ease on the performance of paracentesis. One of them, tapped on the 12th September, was still living in a condition of comparative ease. There was in the cavity of the abdomen a certain quantity of fluid, but it was not great in amount, and it had not increased for the last five or six weeks. One of the other cases happened to be a private case. It was also tapped on the 12th of September, and went on well for the following couple of months. Then owing to one of those fluctuations in the sensuous private patients, which they all knew to be common in chronic cases, other advice was sought, and the patient was tapped a second time without any communication with him. No doubt the operation was skilfully performed, but the case terminated fatally within a week or ten days of the second tapping. The third case was still to the good, and did not require tapping since that time. Two other cases were somewhat remarkable, as instances of very complicated disease. One was that of a female labouring under a combination of disease of the kidneys, and disease of the liver, in whom there was extreme anasarca, a large collection of fluid in the abdomen, and great distress from this distension. Puncture of the legs gave relief for some time, but it was necessary to perform paracentesis. It was performed in all eleven times in that case. She was, for a certain period, under the care of Dr. Banks, and was tapped in his wards three or four times. That case was further complicated; she presented albumen in the urine in large quantity, the urine was of low specific gravity, there was extreme anasarca and a large amount of fluid in the abdomen; and the case also presented partial right paralysis with temporary aphasia, or rather more properly, hetro-phasia—that is to say, she lost the power of expressing her sensations in appropriate words, but used words at random. There was a certain amount of paralysis in the right side of the face, right arm, and right leg; and she recovered from this complicated condition and was greatly relieved by tapping. She had an extraordinary faith in tapping, and on the last day of her existence, the distension of the abdomen being very extreme, and the distress very great from that cause, as well

as from effusion into the pleural cavity, she urgently prayed to be tapped. Having been ten times previously restored almost from death to life, she begged most earnestly that the operation should be again performed, and, yielding to her entreaty, he tapped her on that occasion also. This curious clinical incident occurred, that though there was undoubtedly, as proved by percussion, and afterwards by *post-mortem* examination, a large quantity of fluid in the cavity of the abdomen, penetration of that cavity by the trocar did not succeed in bringing any considerable quantity of fluid away. A few ounces trickled away, but there was not that stream which generally flowed out upon the insertion of the trocar. A little reflection satisfied him the reason was to be found in this physiological condition—that the debility was so great and the vital powers so low, that there was no longer any power of contractility in the muscles surrounding the abdomen, which they knew must be in operation to drive the stream from the cavity with the force it usually issues with. Generally when a patient was tapped the fluid flowed out in considerable force, and the outcome of it was due to the compression of all the parts due to muscular contraction. The loss of vital power, in this case, he considered furnished the explanation why the fluid did not escape.

The next case was curious in this respect, that the child who was the subject of it was the daughter of the last-mentioned patient. She had albumen in the urine, enlarged liver, and chronic cardiac disease, as a loud systolic murmur showed. She was ten years of age, and required to be tapped on three occasions in the space of twelve months. She was still living, and he believed it would be his duty to tap her again in the course of a short time. She was last tapped on the 12th of September. She had now a large quantity of fluid in the cavity of the abdomen, and yet was able to go about and, to some extent, enjoy life. The last case he would mention was that of a female who presented two, if not three, large masses in connexion with the ovaries, of what kind he would not define at present. They were fully eight or nine inches in the long diameter, and three or four in the cross diameter, and, besides these, there was a large quantity of fluid in the cavity of the abdomen. Dr. M'Swiny had had this patient under his care in the Jervis-street Hospital, and he tapped her on one occasion. She had since, on repeated occasions, been under his (Dr. Lyon's) care in the Whitworth Hospital, and he had tapped her seven times. The fluid was extremely thick, almost oleaginous, like the kind of fluid found in ovarian sacs, and yet he had no doubt, from the number of times he had tapped her, that this particular fluid was free in the cavity of the abdomen. She had abdominal tumour besides—whether ovarian or not he could not say—and she had been tapped seven times, the last occasion fully three months ago; and she was living in a condition of comparative ease. He did not pretend that there was anything of a novel character about these cases, but he thought that in a

certain clinical sense they were of some practical interest. It was their duty, as physicians, under all circumstances, to prolong life, and he thought these cases went far to show the value of this simple and comparatively painless operation in preserving life, which, without it, would have been lost. Perhaps the chief point of interest was the occurrence of so many cases in the hands of the same individual at the same time. Out of the seven cases two had died. It was easy to state that they must all die; but he thought it must be admitted that life had undoubtedly been prolonged by tapping in these cases. Some fourteen or fifteen years ago the operation of tapping, in any form, had fallen into much discredit; and he thought, looking at the experience they had recently gained on the subject of tapping for fluid in the cavity of the abdomen, or that of the chest, that many of the persons who died from the accumulation, might have had their lives prolonged for a considerable period, had the operation of tapping been resorted to. It was to be borne in mind, in dealing with these cases, they would find some remarkable instances where the patient had been tapped for a considerable number of times, and where at last the secretion ceased to form, and life appeared to be prolonged indefinitely.

No doubt, if relief had not been given by paracentesis, the patients would have died by the pressure on the vital organs of so large a quantity of fluid. He wished to draw attention to the variability of the secretion in these cases, and the variability of the temperature of the fluid; it varied from one to two or three degrees, and in some cases a pungent feeling was perceptible when the finger was placed in the stream; and though it was apparently very hot, it was not more than 101 degrees. In all cases it was highly albuminous, and in one case it was of a thick oleaginous character; but perhaps the most remarkable feature was the variability in the rate of secretion—one patient secreting regularly sixteen quarts of fluid in three weeks, and another patient not secreting that quantity for several months; and lastly, those cases in which arrest of secretion occurred for a long period—in one fifteen months, and in another thirteen months. These were merely records of clinical facts, and he trusted the Society would accept them as such, and excuse the imperfect manner in which he had brought them forward. His example, he hoped, would induce others to bring forward instances from their clinical experience of cases of a similar character.

The CHAIRMAN said, that no doubt there was a fear in the minds of some practitioners of this operation, and he was glad to find a physician of Dr. Lyons' experience coming forward to abate that apprehension. He agreed with Dr. Lyons, that the operation of tapping the abdomen was not as dangerous or as formidable as it appeared to be. He had seen it done often with the best consequences. The mere pressure of so

large a quantity of fluid in the abdomen was in itself a disease. It interfered with the digestion, with the renal secretion, and other vital functions; it was a positive evil, and if the operation of removing that fluid could be performed without adding to the patient's danger, and still more, if it could be done with a prospect of prolonged life, let it be done. The introduction of the aspirateur for the performance of this operation modified the danger. He had applied it to removing fluid from the chest with the most satisfactory result, and he hoped to bring the subject before the Society on some future occasion.

DR. HEAD wished to make a few remarks on those cases of dropsy in which heart affection was the first cause, where the liver became affected, causing portal obstruction. In those cases the secretion went into the blood, and the narrowed orifice was unable to carry it away. In one or two cases of this kind he had tapped with advantage, and in other cases where the dropsy did not manifest itself by an accumulation of fluid in the cavity of the chest he had bled with great advantage. In one case where death was imminent, both from dropsy and pulmonary apoplexy, the patient got a violent attack of bleeding from the bowels, and was so much relieved that the heart symptoms almost ceased to trouble him, although the murmur still existed, and the characteristic pulse. He thought it was desirable to tap early in those cases, before the abdomen was much distended. He had met with a case where a lady had enlargement of the liver and of the spleen. She was tapped twice, and now she remained without any large amount of dropsy. The distension was so great the first time she was tapped that the breathing became difficult, the heart was pressed up as high as the second rib, and she had but little respiratory power, and if not tapped within seven or eight days she must certainly have died. She had not been tapped for a considerable time, and was now in a comparatively good state of health.

DR. LYONS asked if Dr. Head had ever met with a case where the fluid would not come out after tapping?

DR. HEAD replied in the negative. He thought Dr. Lyons' explanation was hardly tenable. In all probability some adhesions had formed between a portion of the abdomen and its contents, and the trocar may have gone into a part where the quantity of fluid was small, but if the abdomen were perfectly free from adhesions, and a trocar was passed into it, on the simplest hydrostatic principles the water must flow out, no matter how flaccid the muscles surrounding the cavity might be. The water would flow out of the bladder of a dead man if it were full and if a trocar were passed into it.

DR. LYONS had anticipated this objection, and had therefore passed a bent probe into the cavity, and it passed about freely, and the fluctuation was so extreme that there could be no doubt there were no adhesions. He doubted whether in all cases when an instrument was introduced into the bladder after death the water would flow out.

DR. HENRY KENNEDY said:—I have listened with very great pleasure to the cases detailed by Dr. Lyons. All of them have some practical bearing. I may state that I knew of two cases where a complete recovery took place after abdominal dropsy had existed some time. One was a female, who was tapped by myself three times, after which the cure occurred, and four years later she remained perfectly well. I have also known similar cases to those called by the author “stationary dropsy,” though none where the state was present in so marked a degree as in his case. I am aware too that Dr. Stokes is familiar with such cases; nor is it uncommon to meet instances where the patients can go about for a long period, and this at a time when ascites exists. Speaking of the number of times a patient may be tapped, one instance occurred to me where a woman had been so treated upwards of eighty times, and I believe in the same patient it was performed upwards of one hundred times before she died. This case was in the North Union Workhouse, under the care of Dr. Kirkpatrick, now the President of the Royal College of Surgeons. In alluding to the treatment of dropsies, I may remind the meeting of the very valuable paper of the late Dr. John Crampton, in which a large number of cases are detailed where the antiphlogestic treatment, including general bleeding, was put in force, and with the best results.

DR. FINNEY asked whether Dr. Lyons had noticed if the fluid had altered its character, whether it had become more thin or otherwise at the thirty-sixth tapping than it was at the first? He also asked whether the condition of the spleen had been observed in these cases? In many cases of enlargement of the liver, where the portal circulation was obstructed, the spleen became enlarged, but in those cases where there was much dropsy it was found the spleen was small. In the cases where the bowels poured out blood the spleen was found to be very much enlarged.

DR. EAMES said that he was not aware, that amyloid disease of the liver, *per se*, caused abdominal dropsy. That if such dropsy did occur, it was due to secondary degeneration and enlargement of the glands causing pressure on the vena portæ. He would be glad to learn from Dr. Lyons what was the pathology of the bossy enlargements of which he spoke. Dr. Eames also thought that the temperature of 101° Fahr. was

a high one for the ascitic fluid, and wished to know whether observations as to the temperature had been made previously to the operation. Dr. Finney had alluded to enlargement of the spleen in connexion with cirrhosis of the liver. Some time since he (Dr. Eames) saw a case of ascites, with cirrhosis of the liver, in which the spleen was enlarged, and it was interesting to remark that after each tapping the spleen became smaller. After death the spleen was found in quite a shrivelled condition, and on speaking to Professor Smith he said he was well aware of such condition of the spleen, the result of repeated tapplings.

DR. GORDON said the cases brought forward by Dr. Lyons showed that these repeated tapplings could prolong life, and even taking the negative side of the question, the cases were exceedingly few in which any objection could be taken to them. Perhaps a good deal of the success which latterly attended the operation was owing to the manner in which it was performed. He remembered seeing a patient taken out of bed, swathed round, carried to the operating table, and he had seen fatal results from this mode of proceeding. Now, however, the operation was performed in the gentlest manner, the patient lying quietly in bed, and the result was that danger was greatly diminished. The real practical question was, whether in those most usual cases, where ascites was connected with cirrhosis of the liver, should the operation be performed early, or should they wait until it became absolutely necessary? All the English writers agreed that it should not be until it became necessary from pressure on the internal organs. That was not his experience. He had seen a great many cases in which the urgency of the symptoms had been considerably allayed, and a healthy condition of the organs maintained for a very considerable time by early operation.

DR. M'SWINEY fully agreed with Dr. Lyons as to the advantages resulting from the operations of paracentesis; there were many who held with him, that it was desirable to let out these fluids in all these cases, whether they were infused in the chest or abdomen, as soon as possible. He had been in the habit of carrying that doctrine into practical operation in his hospital, as much as he could. Hence it happened, that as soon as a woman presented herself in a most prostrate condition, he had the fluid removed from what he thought was an ovarian dropsy. But he did not confine himself to cases of that kind. In cases of advanced dropsy from cirrhosis of the liver, where death seemed imminent, he had had the fluid removed with marked benefit to the patient. He wished to remark, that a *post-mortem* revealed in one instance that death was due directly to peritonitis excited by the operation. In that instance Dr. Robert M'Donnell operated on a patient of his, and he mentioned Dr. M'Donnell's name to show the operation was conducted in the most scientific and skilful manner. Dr. Lyons did not allude to any danger of that kind. He (Dr.

M'Swiney) was at a loss to understand why the fluid had not flowed out, and he would ask him whether the tightening and compressing of the bandages would not sufficiently compensate for the absence of vital contractility of which he spoke. He had witnessed two cases in the practice of the late Mr. O'Farrell, in which he had operated at least twenty-seven or twenty-eight times, with the result of prolonging life, almost indefinitely. And in the Incurable Hospital, Dr. Geoghegan had pointed out to him a case in which he had operated some forty times, with most satisfactory results.

DR. WM. MOORE did not know whether Dr. Lyons had used the improved instruments by which the risk of peritonitis was reduced to a minimum.

DR. LYONS in reply said he had used nothing but the ordinary trocar, and the amount of pain it produced was inconsiderable. He thought it was an object to draw away the fluid with a moderate degree of rapidity. He put the patient on the hepatic side, which caused the liver to fall down, so as not to interfere with the operation. He penetrated below the umbilicus and with a little firm quick pressure, there was no difficulty in passing in the trocar. The fluid flowed easily, but even with a moderate-sized instrument, it took from 25 to 35 minutes to evacuate 16 quarts. When the fluid was evacuated the patient sometimes became weak, but was easily restored by stimulants, and not the slightest alarming symptom occurred in any of the cases he had mentioned. The poor woman who died, was dying at the time, and he performed the operation at her urgent request, and not with any hope of saving her. He fully agreed with Dr. Purdon as to the importance of early tapping, before the vital powers were run down. Some years ago it was extremely difficult to get consent on consultation to the operation of paracentesis, and patients had been allowed to die because it had not been performed. He believed that experience was in favour of the operation, and he did not know of any serious dangers resulting from it. In none of the cases he had mentioned was the spleen enlarged, and it was not enlarged in the chronic case, which had been tapped 37 times. He could not detect any change in the character of the fluid. Dr. Eames had drawn attention to the temperature. Now on the 22nd August, 1871, the woman whose case he had first mentioned was tapped for the 37th time, when 10 quarts were drawn away, making in all 410 quarts. She was then in a state of extreme distress, and the temperature, as recorded by his then clinical clerk, now a medical officer in the West Indies, as being 108, a most remarkable amount. With regard to the bossy elevations on the liver, he could not give them any information, as the patient was still alive.

The Society then adjourned.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIFTH ANNUAL SESSION.

JOHN R. KIRKPATRICK, M.B., Honorary Secretary.

Saturday, 11th January, 1873.

The Vice-President, DR. ATTHILL, in the Chair.

The following Report of the Rotunda Lying-in Hospital, for the year 1872, was read by GEORGE JOHNSTON, M.D., F.K. & Q.C.P., Master :—

GENTLEMEN,—In submitting to you this, my fourth annual clinical report of the Rotunda Lying-in Hospital, you will excuse me if I beg leave once more, for I feel I am compelled, to recapitulate what I have said on each previous occasion, that “in bringing forward these reports I have no theory of my own to promulgate, no other object whatever but that of simply eliciting truth, and by giving you a plain, unvarnished statement of facts, as they occurred during the year, you may be the better able to test the question, whether a large maternity is, or can be made, a safe asylum for those seeking its advantages.”

At the same time, I must maintain, and I am sure that all unprejudiced minds will allow, notwithstanding what has been said to the contrary, that in considering the sanitary state of our institution, the statistics of small country hospitals bear no analogy whatever with those of large maternities, the great majority of the patients admitted into the former being a chosen few, people of good character, recommended as such by the patrons of the charity, or if perchance some unfortunate should seek and gain admission, she, in all probability, is so callous and indifferent to her state, that she cares not to brave out her guilt with shameless and unblushing face, in the midst of her friends, or in the locality where she is so well known.

Whereas in such an institution as the Rotunda they come from all quarters of the city, the country, and occasionally from England and other parts of the British Dominions, and even America has contributed her quota, specially to be confined in our institution. The victim of seduction, the houseless stranger, the famished wretch, all seeking admission may enter at any hour, night or day, without either note or ticket of

recommendation, their only requirements being, that they stand in need of our assistance, a circumstance which, so far as I am aware, is peculiar to this institution. The modest girl, who, having been led astray, and acutely sensitive of her fallen state, flying from the observation of her family and friends, in order to avoid the scandal and opprobrium that she would be exposed to were she to remain in her own home or neighbourhood, seeks the shelter of the Rotunda, where unknown, among the multitude, she hopes to elude observation.

Women deserted by their husbands, or who have been left destitute by their partners having fallen victims to the many diseases always so prevalent in large cities, but particularly within the past year, leaving the widow in a state of mind often bordering on distraction, themselves and families being in a state of penury, not knowing where to look for succour. Others again, who, having been under the care of some unskilled person in their own homes, and whose case becoming serious, or getting beyond the capacity of those in attendance upon them, and who, mayhap, after trying all their efforts to effect delivery, without success, send them to the hospital often in a most deplorable, nay, hopeless state. Some labouring under acute complaints, such as pneumonia, bronchitis, laryngitis, typhus or typhoid fever, scarlatina, erysipelas, &c., and whom we are obliged to retain, their state being such as to prevent their being moved to another hospital. Such cases are extremely liable to be attacked with puerperal fever or peritonitis, from which, in addition to the original complaint, being more than the system can bear up against, they too frequently succumb. And should a fatal termination ensue, ought their deaths to be attributed to their having been confined in the hospital, or would not the same result be as likely to occur, though their confinement had taken place in their own homes, or had they been removed to isolated dwellings? We too well know that in private, even among the upper classes, with every advantage of purity of air, and every luxury at their command, where such complications occur, how frequently death is the result. And here, if I may be allowed to express my opinion, I will say, and that advisedly, that from the closest observation of the cases admitted into hospital for the last four years, I feel assured that in *no one instance* could the death be in any measure ascribed to either its air or influence.

On the contrary, I can bring forward many instances of patients, such as the poor seduced girl, or woman labouring under great distress of mind, who, although their labour was perfectly natural in every respect, began to show symptoms of the worst character, when by cheering them up, and holding out prospects of protection for themselves and their offspring, have rallied, and made a favourable recovery. Others again, who had most difficult labours, requiring our greatest efforts to effect delivery, have progressed most satisfactorily, and went out quite well;

while others whose labours were perfectly natural and of short duration, and who were progressing favourably, on receiving some sudden shock, by bad news imprudently conveyed to them, were immediately attacked with inflammatory symptoms of the most serious character, and, notwithstanding the most prompt and active measures, they succumbed. Should such an event happen, surely it ought not to be attributed to their having been confined in the hospital, while in the former instances strong proof is evinced that there is nothing of a noxious quality in its atmosphere.

And here I may take the opportunity of stating, that notwithstanding the fearful epidemics which prevailed during the past year, and although several patients were admitted from infected houses, and many with disease absolutely upon them, it never extended beyond the one individual. In fact, the hospital was perfectly free from any contagion.

During the year ending 5th November, 1872, there were 1,193 deliveries took place in the hospital, 130 cases delivered at their own homes, 3,677 attendances at the dispensary, 289 treated in the chronic wards, making in all 5,289 cases relieved in the year. Of the 1,193 intern labour cases, 426 were primiparæ, and 767 pluriparæ; 931 were purely natural labour, i.e., the head presented, and the delivery was effected by the natural efforts within twenty-four hours, 316 of which were primiparæ, of whom four died—one of pyæmia, having had symptoms of delirium tremens; one of bronchitis, from which she had been suffering for some months, and in whom peritonitis supervened; one of peritonitis, which set in twenty-four hours after confinement, being seduced, and fretting greatly, and one of peritonitis, which also set in twenty-four hours after confinement, with great prostration of strength and very low spirits, "husband having deserted her."

There were two cases where the labour exceeded twenty-four hours, but the child was born by the natural efforts, one, her first, thirty hours in labour, six occupying the second stage; the second, her second child, twenty-eight hours in labour, the second stage occupying only half-an-hour; both recovered.

In 49 patients the ovum was expelled prematurely, viz., three in the sixth week, six in the second month, twenty in the third month, four in the fourth, six in the fifth, and ten in the sixth month. Three of the 49 abortions were primiparæ, viz., two at three months and one in the fifth month; all the mothers recovered.

In six cases the child presented with the upper extremity, viz., two the elbow being the presenting part, one being primipara; in two the hand presented; in one the shoulder, a primipara, and one, her seventh pregnancy, the arm was protruding through the vagina on admission. In all version was performed, under the influence of chloroform, with the exception of the shoulder presentation, which was the second of

twins, and all recovered but the case of arm presentation, the uterus having been ruptured previous to admission.

In 33 instances the breech or lower extremity was the presenting part, 7 being primiparæ, 26 pluriparæ; 20 were breech presentations; 4 of which were in twin cases, 2 being primiparæ. In 13 instances the foot presented, 3 being primiparæ; 5 were in twin cases, 1 of which was primipara; all the mothers recovered.

FORCEPS.

In 131 cases it was considered prudent to employ the forceps to effect delivery, 95 being primiparæ, and 36 pluriparæ. This, no doubt, appears a large number, and will be considered by some, followers of the old school, in the light of "meddlesome midwifery."

But having now for some time closely watched the process of labour, and carefully considered all the circumstances attendant upon the descent of the foetal head through the pelvis, the injurious effects produced by its long pressure on the soft parts, and in cases where the liquor amnii has escaped at the commencement of labour, the danger that arises from the head pressing on the expanded cervix uteri, before the os is fully dilated, we have come to the conclusion, and our established rule is—that so long as nature is able to effect its purpose without prejudice to the constitution of the patient, danger to the soft parts, or the life of the child, we are in duty bound to allow the course of labour to proceed; but as soon as we find the natural efforts are beginning to fail, and after having tried the milder means for relaxing the parts, or stimulating the uterus to increased action, and the desired effects not being produced, we consider we are in duty bound to adopt still prompter measures, and by our timely assistance relieve the sufferer from her distress, and her offspring from an imminent death. Why, may I ask, should we permit a fellow creature to undergo hours of torture when we have the means of relieving them within our reach? Why should she be allowed to waste her strength, and incur the risks consequent upon long pressure of the head on the soft parts, the tendency to inflammation and sloughing, or the danger of rupture, not to speak of the poisonous miasm that emanates from an inflammatory state of the passages, the result of tedious labour, and which is one of the fertile causes of puerperal fever and all its direful effects, attributed by some to the influence of being confined in a large maternity, and not to its proper source, *i.e.*, the labour being allowed to continue till inflammatory symptoms appear. The more we consider the benefits of timely interference, and the good results which follow it, the more are we induced to pursue the system we have adopted, and to inculcate to those we are instructing the advantages to be gained by such practice, both in saving the life of the child, as well as securing the greater safety of the mother.

At the same time we do not forget to point out that although the forceps in the hands of the skilful practitioner may be a perfectly safe and innocuous instrument, when used cautiously, and with due regard to the internal conformation of the pelvis; that on the contrary they become the very reverse, when attempted by those not thoroughly acquainted with the mechanism of parturition, or who have not acquired that sensibility of touch which is so essential to the obstetrician. That although to the looker on, their application may appear simple and easy, still, that the greatest care and caution is required in the mode of their introduction, the accuracy of their application, and eventually in the method of extraction.

Of these 131 cases, there were 9 deaths, or 1 in 14½, viz., 1 from ruptured uterus, her third pregnancy, having been allowed to remain ten and a-half hours in the second stage; she died in twenty hours after delivery; 1 from the island of Jersey died of mental distress, being seduced, her first pregnancy, no abdominal symptoms whatever were present, as corroborated by *post-mortem* examination; 2 from convulsions, viz., 1 a primipara had 27 fits, and died on the fifth day, and 1, her fifth pregnancy, admitted in her seventh month, when labour was induced, convulsions continued, and she died within thirty hours; 1 died of peritonitis, a primipara, in very delicate health, fretting, "her husband being at sea;" 1 died of peritonitis, but with extreme disease of the kidneys, a primipara, and unmarried; 1 died of peritonitis, a primipara, aged forty, sent from the country in a very delicate state of health, with fatty degeneration of the heart; 2 died of gastro enteritis, both primiparæ, and both suffering from gastritis on admission, 1 of which was a case of seduction.

77 male and 54 female children were delivered, of which 62 male and 42 females were born alive; 5 males and 7 females died some time after birth; 6 males and 4 females were dead when born, and 4 males and 1 female were putrid.

In 35 instances we were obliged to employ the forceps before the os was fully dilated, 27 being primiparæ and 8 pluriparæ. In 30 of these cases the interference was considered necessary, in consequence of the os uteri continuing undilated, apparently the result of the too early rupture of the membranes, and the escape of the liquor amnii, and thus exposing the cervix to an injurious amount of pressure, and which, if prolonged, would be so apt to produce sloughing, and mayhap fatal results.

In all these cases the usual means of dilating the os were first employed, and as soon as it was sufficiently expanded to enable us to pass the blades of the forceps, we did not hesitate to employ them, and in every instance, so far as the labour was concerned, with a beneficial result. All the mothers recovered but 2, both primiparæ;

in 1 the waters had escaped 7 days; she was an elderly woman, aged 40, in very delicate health, with gastritis and disease of the heart, and although very anxious about herself, she went on favourably for four days, when peritoneal symptoms set in, and she died on the ninth day. At *post-mortem* examination peritonitis to a slight extent was found; the uterus was healthy. The second was admitted with gastritis, diarrhoea supervened, which could not be checked, and she died on the fifteenth day.

In 3 cases the interference was owing to convulsions; 2 primiparæ recovered; 1, her fifth pregnancy, died.

In 2 from accidental hæmorrhage, both pluriparæ, and both recovered.

22 male children were delivered, 17 being alive, all of which lived. 12 female children were delivered, 11 being alive, 9 of which lived

In order to show the result of the practice more clearly I refer you to the following table (p. 188).

CRANIOTOMY.

In 5 instances we found it necessary to lessen the head before delivery could be effected, 4 being primiparæ, and 1 pluripara; in 2 cases it was owing to narrowing of the transverse diameter, both being primiparæ, 1 of which was complicated with convulsions, and who died; the other recovered. In 1 case, her fourth pregnancy, it was in consequence of narrowing of the antero-posterior diameter of the brim. She recovered. There were 2 cases where the pelvis was diminished in size (*pelvis simpliciter justa minor*), both primiparæ; 1 recovered, and 1 died of gangrene of the uterus, with extensive renal disease.

In all the forceps were tried previous to craniotomy. In 3 cases the cephalotribe had to be employed, in order to extract the head, and in all there was undoubted signs that the child was dead before the operation.

VERSION.

Version was performed in 21 cases, 8 being in primiparæ; 13 in pluriparæ, in 5 being the second of twins. 8 cases were presentation of the upper extremity; 1 a case of convulsions, 3 placenta prævia, 1 accidental hæmorrhage, 2 prolapse of the funis, 1 malposition of the head. 5 mothers died, 3 being primiparæ, viz., 1 a case of convulsions; 1 seduction, died of mental distress; 1 of disease of kidneys, with gangrene of the uterus mentioned before; 2 pluriparæ; 1 of hæmorrhage from placenta prævia; and 1 a case brought in from the country with ruptured uterus.

TWINS.

In 19 instances the labour was complicated with twins, 6 being primiparæ, and 13 pluriparæ. 1 mother, a primipara, died, as before mentioned, of mental distress, being seduced.

Cases where the Forceps were Used before the Os Uteri was fully Dilated.

No.	Age	No. of Pregnancy		CHILD					Hours in Labor	Cause of Interference				Result to Mother		Observations
		1st	Subt	Sex		Weight	Lived	Died		Early rupture of membrane	Accidental Hemorrhage	Con- vulsions	Recovered	Died		
				M.	F.											
1	21	1	—	—	1	9 4	—	D. 4th day	36	1	—	—	1	—	Pulse getting quick; tongue dry.	
2	26	1	—	1	—	6 8	1	—	36	—	—	1	1	—	1st of twins, 2nd delivered by version.	
3	19	1	—	1	—	6 10	1	—	10	—	—	1	1	—	Great exhaustion. Convulsions in 7th month; labour induced.	
4	27	—	3	—	D.	6 12	—	—	7	—	1	—	1	—		
5	21	—	5	—	1	2 8	—	D. 34 hours	—	—	—	1	—	1		
6	23	1	—	1	—	7 6	1	—	35	1	—	—	1	—	Head in 3rd position.	
7	39	—	9	—	1	7 12	1	—	25	1	—	—	1	—	Left parietal bone of infant depressed to the extent of $\frac{3}{4}$ inches by $\frac{1}{4}$ inch, and to a depth of $\frac{1}{2}$ of an inch, in consequence of an exostosis to the right of sacro-iliac promontory.	
8	23	1	—	—	1	7 4	1	—	46	1	—	—	1	—	43 hours in labour before admission.	
9	30	—	2	D. P.	—	6 12	—	—	26	1	—	—	1	—	Admitted in very delicate health; gastritis; disease of heart.	
10	43	1	—	1	—	6 12	1	—	23	1	—	—	1	—		
11	27	1	—	—	1	6 9	1	—	30	1	—	—	1	—		
12	21	1	—	D.	—	7 11	—	—	30	1	—	—	1	—		
13	25	1	—	1	—	7 10	1	—	26	1	—	—	1	—		
14	40	1	—	1	—	6 10	1	—	24	1	—	—	—	1		
15	34	—	2	—	1	7 0	1	—	26	1	—	—	1	—		
16	25	1	—	1	—	7 14	1	—	27	1	—	—	1	—	Projection of sacral promontory.	
17	32	1	—	1	—	9 0	1	—	30	1	—	—	1	—		
18	37	—	9	1	—	7 12	1	—	15	—	1	—	1	—		
19	23	1	—	—	1	7 9	1	—	21	1	—	—	1	—		
20	20	1	—	1	—	7 4	1	—	21	1	—	—	1	—		
21	24	1	—	—	1	6 0	1	—	21	1	—	—	1	—		
22	35	—	8	—	1	6 8	1	—	16	1	—	—	1	—		
23	30	1	—	D. P.	—	7 0	—	—	30	1	—	—	1	—	In very delicate health.	
24	25	1	—	1	—	6 11	1	—	15	1	—	—	1	—		
25	27	1	—	—	1	6 10	1	—	20	1	—	—	1	—		
26	25	1	—	1	—	6 5	1	—	12	1	—	—	1	—		
27	20	1	—	1	—	7 13	1	—	20	1	—	—	1	—		
28	42	—	2	D. P.	—	7 8	—	—	20	1	—	—	1	—		
29	25	1	—	1	—	8 0	1	—	15	1	—	—	1	—		
30	25	1	—	1	—	7 2	1	—	36	1	—	—	1	—	Membranes ruptured 3 days. Disproportion; head in 2nd position; pulse weak intermittent.	
31	29	1	—	1	—	8 5	1	—	38	1	—	—	—	1		
32	30	1	—	D.	—	7 6	—	—	33	1	—	—	1	—		
33	25	1	—	—	1	7 2	1	—	40	1	—	—	1	—		
34	22	1	—	—	1	7 4	1	—	24	1	—	—	1	—		
35	20	1	—	1	—	6 12	1	—	28	1	—	—	1	—		
	27	8	—	22	13		27	2	—	30	2	3	32	3		Disproportion; had gastritis on admission; took enteric form.

ACCIDENTAL HÆMORRHAGE.

We had 10 cases of accidental hæmorrhage, all being in pluriparæ, and all recovered. In 7 instances rupturing the membranes was sufficient to restrain the hæmorrhage; in 2 we were obliged to deliver by the forceps, and in 1 by version. 4 children lived; 5 were dead at birth. 1 was putrid. All the mothers recovered.

UNAVOIDABLE HÆMORRHAGE.

There were 4 cases of unavoidable hæmorrhage, all in pluriparæ; 3 were cases of partial placenta prævia—they recovered;—1 was a case of complete; and admitted in a state of great exhaustion; was plugged, and as soon as the os was sufficiently dilated, version was performed; her strength supported, but she died in 24 hours.

POST-PARTUM HÆMORRHAGE.

There were 6 cases of *post-partum* hæmorrhage; 2 in primiparæ; 4 in pluriparæ; 5 being of a trivial character, 1 only requiring the injection of the solution of the perchloride of iron; all but 1 recovered, and went out well on their eighth day. The fatal case was one of purpura hæmorrhagica, aged 39, her third pregnancy; admitted in a state of great exhaustion; her labour was natural; the placenta was retained from inertia, with some hæmorrhage, but so slight that it was immediately restrained by the injection of cold water; however, from her previously exhausted condition, together with her disease, she gradually sank, and died on the ninth day.

RETAINED PLACENTA.

There were 2 cases where the placenta was retained; 1, from irregular contraction, a primipara; she died from uremia. 1, from morbid adhesion, a pluripara; child born in the country 40 hours before admission. Chloroform had to be administered before we were able to effect its removal. She recovered.

PROLAPSE OF THE FUNIS.

Prolapse of the funis occurred in 7 instances; 8 in primiparæ; 4 in pluriparæ; 3 boys and 1 girl were born alive; 2 boys and 1 girl were dead at birth. All the mothers recovered.

CONVULSIONS.

There were 5 cases where the labour was complicated with convulsions, all being primiparæ. 1 was a case of twins, first child, a male, delivered by forceps. The second, a girl, by version. Both children lived, and the mother recovered. The second case was also delivered of a boy, which lived, and the mother recovered. The third had 16 fits before delivery, in which, after version had been effected, obliged eventually to perforate the head, and

complete extraction by the crotchet; mother died. The fourth was sent in from the country, had 4 fits before labour, which had to be assisted by the forceps; the child, a boy, was dead; mother also died. The fifth had 3 fits before admission; delivered by the forceps of a girl living; mother died.

RUPTURE OF THE UTERUS.

We had 2 cases where the uterus was ruptured before delivery. 1, her third child, was allowed to remain $10\frac{1}{2}$ hours in the second stage before we were sent for, head having been arrested at the brim for $9\frac{1}{2}$ hours; the long forceps were applied, and the head delivered with considerable difficulty, after which the shoulders had to be extracted by the blunt hook; the child, a girl, was dead, and weighed 10 lbs. 6 oz.; mother died in 21 hours. The second was her seventh pregnancy, brought in from the country, with the arm protruding through the vulva for 12 hours; uterine action had ceased; delivery was immediately effected by version; the child, a boy, was dead, weighed 8 lbs.; mother died in 25 hours.

CHLOROFORM.

Chloroform was used in 131 cases, and in all, so far as the anæsthetic influence was concerned, with favourable results, nor in any was there *post-partum* hæmorrhage. In every case before its administration we took the precaution of giving a dose of ergot.

PERITONITIS.

There were 14 cases of peritonitis; 12 being in primiparæ, and 2 pluriparæ; 8 recovered; 6 died; 9 were found to be labouring under great mental distress, 7 being unmarried, and 2 from their husbands having deserted them; 4 of these died. Of those who recovered, in 3 instances we attribute the favourable result mainly to their anxiety of mind being relieved. 1 of the fatal cases was admitted with acute bronchitis; another was admitted with acute gastritis and heart disease. 1 was admitted with unavoidable hæmorrhage and acute bronchitis. 1 came in "bruised all over," as she expressed herself, from a beating her husband had inflicted on her.

PYÆMIA.

There were 2 cases of pyæmia. 1, aged 30, her first pregnancy, was a case of tedious labour, lasting 50 hours, the delay being in the first stage, owing to early rupture of the membranes. Delivery had to be effected with the long forceps, the head being high up in the cavity; her convalescence was slow, owing to general delicacy; a pyæmic patch appeared on the right elbow on the tenth day, with low feverish symptoms; under treatment she gradually, but slowly recovered, and was discharged on thirty-first day.

The second, aged 24; her third pregnancy, was admitted in very delicate health, from want, suffering from acute bronchitis, with dyspnoea, fretting greatly, her husband having deserted her; confined December 20th, 1871, after a natural labour of 10 hours' duration, second stage lasting $1\frac{3}{4}$ hour; on the 24th a pyæmic patch appeared on the knee; her cough very troublesome; was put on bark with chlorate of potash; on the 29th she was moved to the chronic ward for change of air; on the 31st great difficulty of breathing, pneumonia at base of left lung, rusty sputa, for which she was treated, and the pulmonary symptoms improved, but the knees continued swollen and painful, with great prostration of strength; on the 3rd January phlebitis appeared in right leg, foot swollen and painful, which under treatment also subsided; however, it was followed by sloughing over the sacrum from constant supine posture, not being able to lie on either side, owing to the intense pain excited in the knee; she was put on an air cushion, and the other patients were moved out of the ward, that she might have a greater amount of pure air; subsequently psoas abscess formed, which burst on the 30th May, and she gradually sank in a state of great exhaustion on the 11th June, nearly 6 months from the time of her delivery.

PHLEBITIS.

1 case of phlebitis occurred in a patient who was admitted with, and had been under treatment in the hospital since the 18th January, 1872, for intense neuralgia of the uterus, being at the commencement of the eighth month of her fourth pregnancy. She was confined on the 6th March, after a natural labour of 9 hours' duration, second stage occupying 2 hours, of a female child, weighing 9 lbs. 12 ozs. Within 24 hours she was seized with pain in her left leg, with intense tenderness along the course of the vein. The usual treatment was adopted, with beneficial effects. Her recovery was slow, but perfect, and she was discharged completely well 7 weeks after her confinement.

GASTRITIS.

We had 3 cases admitted with acute gastritis. In 2 enteritis supervened, in 1 of which peritonitis to a slight extent followed. Both died. In 1 it was combined with disease of the heart, and peritonitis to a slight extent supervened. She died.

SCARLATINA.

Scarlatina appeared in 3 instances, viz. :—1 on the 18th January, in No. 2 ward. She was unmarried, aged 23, her first pregnancy; confined the 13th of a boy, living; weight 6 lbs. 12 ozs.; went on favourably till her fifth day, when a roseolar rash appeared over her neck, body, and legs. She was moved to the Hardwicke Hospital. Four other patients

were confined at the same time in the ward, and all, both mothers and children, went out well.

The second occurred in No. 8 ward, in an unmarried woman, aged 16, her first pregnancy. She was confined on the 23rd March of a boy, living, weighed 7 lbs. 12 ozs.; went on favourably till the third day, when the roseolar rash appeared, with sore throat. She was moved to the Hardwicke Hospital. 4 other patients were confined at the same time in the ward, and who, with their children, were discharged convalescent.

The third appeared in No. 4 ward, on May the 11th, in a patient aged 18, her first pregnancy, who was confined of a girl, living, weight 5 lbs. 12 ozs. On the 15th redness appeared over the neck, chest, arms, and legs, and in the evening the rash was fully developed. There being only 2 other patients in the ward at the time, and as a space of 1 bed intervened between them and her, we allowed her to remain. She progressed favourably; desquamation took place in due course, and she was discharged quite well. The other patients in the ward went favourably through their convalescence, and all the children were perfectly healthy.

VARIOLA.

We had 10 cases of variola within the period of this report, viz.:—9 among the labour patients, and 1 in one of the inmates, who had no intercourse with the wards. She was moved to the Pension Medicale, where she died on the eighth day from the attack.

In 1 case, the woman was labouring under the disease when she applied for admission, and as her labour was far advanced, her first pregnancy, she could not be sent away, and was delivered, shortly after getting into the ward, of a healthy boy, weighing 7 lbs. 11 oz. It was considered prudent to move her, as soon as possible, to the Hardwicke Hospital.

In 7 cases the disease was latent, 1 not showing itself till 6 hours after delivery; 1 in 12 hours after admission, not in labour; 1 in 24 hours after delivery; 1 in 26 hours after delivery; 2 not until 3 days after admission; and one of these had not been delivered. In one instance, the disease did not show itself till the fourth day after delivery. 6 of these were sent, 4 to the Hardwicke and 2 to the Mater Misericordiae Hospitals. The 7th case, second pregnancy; labour natural, went on favourably till the fourth day, when spots, of a modified form, were noticed on her arms, face, and body. She was moved to a separate small ward, where, under treatment, she went on favourably, and was discharged quite well.

In one instance the variola appeared in the infant.

The mother, unmarried, fretting, her first pregnancy, otherwise in good health, was delivered, after a natural labour of 8 hours' duration, of

a boy, healthy, in good condition, weighing 7 lbs. 2 oz. Mother's convalescence was favourable, but slow, in consequence of her mental distress. On the ninth day a purple rash appeared upon the child's forehead, had been perfectly healthy from its birth till now, became low and weak, was given wine-whey. On the following day small petechial spots appeared on the shoulders and neck, with a regular variolous vesicle over the left temple, and several smaller ones on the buttocks and legs, with great prostration of strength. He rapidly sank in 46 hours after the first appearance of the rash. The mother continued perfectly healthy; had two well-marked cicatrices on her arm. She had not been re-vaccinated.

All these cases were more or less in contact with the other labour patients in whose wards they were for a period, varying from 6 hours to 4 days, and yet in no instance did the disease extend beyond the one individual. And, that it may be seen that these cases did not take place simultaneously, I now give them to you in the order of time in which they occurred. Thus, the first was on the 15th December, 1871, in No. 5 ward, 3 patients having been confined the day previous in the same ward, and all went out well. The next case that occurred in that ward was on January 19th, 1872, and was a woman who complained of pain in her back and loins; tongue loaded; pulse 112; with suspicious spots on her face and hands; not in labour. She was sent to the Hardwicke Hospital. In the interval, viz., on the 8th January, 4 patients were confined in the ward, and all went out well; and 5 patients who were confined at the time, also made a favourable convalescence.

The third case occurred in No. 6 ward, on January the 9th; this patient was admitted complaining of pain, but not in labour, on January 7th, into No. 4 ward, where 4 patients were confined, all of whom went out well. She was transferred on the day following, not being in labour, to No. 5 ward, where 4 patients were also confined, and all convalesced. She was then moved to No. 6 ward, but on being transferred to it, on the 9th, a varioloid rash appearing over her arms, neck, and body, she was sent to the Mater Misericordiæ Hospital. 4 patients had been confined at the time that this patient remained in the ward, and all went out well.

The fourth case was in No. 3 ward, and was confined on February 22nd; on the 29th, her seventh day, patient complained of pain in her left knee, for which she received the ordinary treatment; on the 3rd of March, her eleventh day, purpurous patches appeared over face, arms, and legs. She was accordingly sent to hospital. 3 other labour patients were in the ward at the same time, all of whom recovered.

The fifth occurred March 24th, in No. 7 ward, was the case of the infant previously mentioned; 3 patients having been confined in the same ward during the time, and all went out well. Nor did any case occur in the ward till June 12th, during which period it was occupied by

eight sets of labour patients, amounting to 33, all of whom went out well. This patient had been in No. 5 ward, when, on the number of labour patients being completed, she was sent to No. 6, having been 24 hours in the hospital before the symptoms appeared. She was not in labour, and was sent to the Mater Misericordiæ Hospital.

The seventh took place on March 27th (her fourth day), in No. 7 ward, and is mentioned previously, as the seventh case, being one of a modified form, was moved into a small ward, and recovered, 3 other patients having been confined at the same time in the ward, and recovered; nor did any case occur in it till the 15th September, when one who had been admitted in a feverish state, and confined the day previous, prematurely (seventh month), showed the disease; she was sent to the Hardwicke Hospital. In the interval the ward was occupied by 15 sets of labour patients, amounting to 57, all of whom recovered favourably.

The ninth took place in No. 4 ward, on the 19th April, having been 2 days in the hospital. She was first admitted to No. 3 ward, where 5 patients were delivered, and all of whom recovered. She was then moved to No. 4 ward, and while there 3 patients were confined, and all convalesced. The eruption appeared shortly after delivery, so was moved as soon as possible to the Hardwicke Hospital.

The particulars of the tenth case have been already given.

There were 24 cases of acute pulmonary affections, viz., 22 bronchitis, 1 laryngitis, and 1 pleuritiâ.

1 case of hæmatemesis, which recovered.

5 cases of syphilis.

2 cases of neuralgia of the uterus prior to confinement. 1 had phlebitis after delivery. Both recovered.

1 case of purpura hæmorrhagica.

1 of fibroid tumour of the uterus; went on favourably through her confinement.

1 of epilepsy.

6 of mania, 5 being unmarried, 2 of whom died; 1 from extreme mental distress, and the other from extensive renal disease.

DEATHS.

20 deaths occurred during the year from all causes, being an average of 1 in $59\frac{1}{2}$; but if we deduct those which died from accidental causes, if I may so call them, and which amounted to 14, it leaves 6 which died of zymotic diseases. This lessens the average to 1 in $198\frac{1}{2}$, and when we take into consideration the circumstances under which they took place, we may fairly say, that they could not in any way be attributable to their having being confined in the hospital. That, in fact, they would have died, even though their delivery had taken place elsewhere.

TABLE No. 1.—Deaths from all Causes, from Nov. 6th, 1871, to Nov. 5th, 1872.

No.	Date of Death	Locality in Hospital		Patient		Cause	Observations
		Ward	Bed	Age	No. of Pregnancy		
1871							
1	Nov.	8	3	23	37	3 Ruptured Uterus	9½ hours in second stage; had to be delivered with the long forceps with great difficulty; child, girl, weighed 10 lbs. 6 oz.
2	"	8	5	46	30	1 Pyæmia	Habits of inebriety, and symptoms of delirium tremens.
3	Dec.	20	1	2	25	1 Convulsions	16 fits before the os was sufficiently dilated to perform version, after which had to lessen the head and complete delivery by the crotchet.
4	"	22	7	64	34	3 Placenta prævia	Perfectly anæmic on admission; died in 18 hours of exhaustion.
1872							
5	Jan.	14	6	70	23	1 Renal disease, Sloughing	Hand presented; waters had escaped 9 days prior to admission; version; after which had to lessen head and deliver with the cephalotribe.
6	"	15	12	116	23	1 Mental distress	Unmarried, from Jersey; fretting greatly, and, as she said, " <i>was dying of a broken heart</i> ;" no abdominal symptom.
7	Feb.	4	5	69	23	7 Laryngitis with Bronchitis	A widow; caught cold attending her husband; great dyspnoea, and aphonia, on admission; gradually sunk.
8	"	6	7	64	23	1 Convulsions	Intense cedema of labia and extremities on admission; 27 fits; delivered by forceps under chloroform.
9	"	23	2	Small Ward	25	5 Convulsions	5 fits before admission; 7th month of pregnancy; labour induced; chloroform; forceps.
10	Mar.	10	1	2	33	4 Mental distress	"Widow;" mania on 5th day; great prostration; gradually sunk on the 9th day; no abdominal symptoms whatever.
11	April	24	5	46	20	1 Peritonitis	Had been in another hospital from injury to her knee; admitted with feverish symptoms; "husband at sea!"
12	May	11	6	68	25	1 Bronchitis, Peritonitis	Had been suffering from bronchitis for several months.
13	"	28	7	64	40	7 Ruptured Uterus	Brought in with arm protruding through the vulva; great pain of abdomen; uterine action ceased; version at once.
14	June	14	12	113	20	1 Uræmia	Unmarried; had to be delivered with the forceps; became semi-comatose; no uterine or peritoneal symptoms.
15	July	6	2	14	40	1 Fatty heart, Peritonitis	Water had escaped 7 days before admission, great delay in consequence; had to be delivered with the forceps; symptoms 5th day.
16	Aug.	23	2	20	24	1 Peritonitis	Unmarried; fretting; labour easy; symptoms set in in 24 hours; died on 8th day.
17	"	29	1	1	27	1 Mental distress	Unmarried; natural labour; great prostration; heart's action very feeble; died in 60 hours.
18	Oct.	8	5	48	30	1 Gastro Enteritis	Unmarried; delivered by forceps; great eructation from commencement; diarrhoea.
19	"	11	4	41	29	1 Gastro Enteritis	Had been in hospital; same complaint 3 months before.
20	"	17	6	69	39	3 Purpura Hæmorrhagica	Great exhaustion on admission.

For the purpose of showing this, I refer you to Table No. 1, which you may perceive is arranged, as I have done in my former reports, in the chronological order in which the deaths took place, with the No. of the ward and that of the bed, showing that where fatal cases from zymotic disease did occur it never extended beyond that one individual. Thus 1 case died of pyæmia in No. 5 ward, bed 46, her first pregnancy, aged 30, delivered 17th October, 1871, after a natural labour, second stage lasting only two hours, of a boy, putrid, in the eighth month, which she attributed to fright a fortnight ago; became very nervous, and had all the symptoms of delirium tremens. Pyæmic patches showed on different parts of the extremities, suffusion of the left eye, with pus in the anterior chamber, and she eventually sank on the 8th of November, on her twenty-second day after confinement. The delivery in this case having taken place the 17th of October, 1871, is recorded in my last report, but as her death did not occur till within the period embraced in this report, it has now to be mentioned.

The next was a case of peritonitis, her first pregnancy, admitted into No. 5 ward, bed 46, 19th April, 1872, 6 months having elapsed since the previous case, during which period 66 patients were confined in the ward, and on 14 occasions the same bed had been occupied, all of whom went on favourably to their convalescence. The report states that on admission her tongue was dry, coated with red edges, had lately come out of an hospital, where she had been for an injury to her knee, from which she was still suffering. "Husband at sea." She had to be delivered with the forceps, in consequence of delay in second stage, owing to disproportion. Symptoms of peritonitis showed themselves within 24 hours, and she died on the fifth day.

The third, admitted May 11th, into No. 6 ward, bed 68, aged 25, her first pregnancy, has been suffering from bronchitis for some months; great dyspnœa on admission; her labour was natural, second stage occupying only half an hour. The dyspnœa continued, and peritoneal symptoms set in immediately, and she gradually sank on her ninth day.

The fourth was sent in from the country in a delicate state of health, from acute gastritis and heart disease, aged 40, her first pregnancy; labour lingering; did not absolutely set in for 5 days after admission; delay in first stage, in consequence of early rupture of membranes, waters having escaped 7 days previous. Warm baths had to be employed, afterwards Barnes's dilator, and as soon as the os was sufficiently dilated, she was delivered with the forceps. Although much prostrated at the time from the effects of the labour she rallied for some time, but on the fifth day began to decline. Peritonitic symptoms showed themselves, and she died on the eleventh day. *Post-mortem* showed, besides slight peritonitis, the heart in a state of fatty degeneration.

The fifth, seduced, aged 24, first pregnancy, fretting greatly; her labour was natural, lasting only 12 hours, quarter of an hour in the second stage; peritonitic symptoms appeared in 24 hours. The usual treatment was adopted, but she gradually got worse, and died on the seventh day.

The sixth, also a case of seduction, admitted with great mental anxiety and prostration, heart's action feeble; labour natural, second stage three-quarters of an hour. Symptoms of peritonitis appeared within 24 hours, became pulseless, and rapidly sank in 60 hours after delivery. *Post-mortem* examination showed peritonitis to a slight degree.

Of the 14 deaths from accidental causes (as they may be called), 2 were from rupture of the uterus; the particulars of these cases have been mentioned; 3 were cases of convulsions; 1 a case of placenta prævia; 2 died from renal disease, one of which had gangrene of the uterus; 3 died of mental distress, without any peritoneal symptom; 2 of gastro-enteritis, one had no peritoneal or uterine symptom whatever at *post-mortem* examination, the other had to a slight extent; and 1 of purpura hæmorrhagica.

In order to give a still clearer account of the number of cases of labour, as they took place in each ward, with the results, and that it may be seen that where a fatal case occurred, it did not communicate its influence to any other patient, I refer you to Table No. 2, which is so arranged that the number of patients delivered in each ward, with the deaths as they occurred in them, are seen by reading the table from above downwards, the primiparæ being printed red; tracing the figures horizontally you will see the number confined in each ward, during the month, with the deaths as they took place in that month; giving the total births and deaths at the end of the line for the month.

Thus you see that in the month of November, 1871, in No. 1 ward, 9 deliveries took place, 1 of which was a primipara. 8 in No. 2 ward, 5 being primiparæ. 17 in No. 3 ward, 3 being primiparæ, and 1 death (pluripara) from an accidental cause. 4 deliveries in No. 4 ward, 2 being primiparæ. 9 in No. 5 ward, 1 primipara, and 1 death, marked Z, being from a zymotic cause, and primipara. 10 deliveries in No. 6 ward, 3 being primiparæ. 11 in No. 7 ward, 2 of which were primiparæ. 11 in No. 8 ward, 5 of which were primiparæ. And 10 in No. 12 ward, 3 of which were primiparæ; making a total of 89 delivered in the month, of which 25 were primiparæ, with 2 deaths, one of which, a primipara, was from a zymotic cause.

Then again, reading the column vertically, you perceive that in No. 1 ward, 126 deliveries took place in the 12 months, 38 being primiparæ, and 3 deaths, all being primiparæ, one of which was from a zymotic cause, which by referring to the Table No. 1, and looking for the month of August, in which it occurred, the circumstances of the case can be ascertained.

TABLE NO. 2.—*Number of Patients delivered in each Ward, with the Deaths, as they occurred in them, during the year ending 5th November, 1872.*

		Ward No. 1		Ward No. 2		Ward No. 3		Ward No. 4		Ward No. 5		Ward No. 6		Ward No. 7		Ward No. 8		Ward No. 12		Total		Deaths from Zymotic diseases	Primiparae		Deaths from Zymotic diseases
		Delivrs.	Deaths	Delivrs.	Deaths	Delivrs.	Deaths	Delivrs.	Deaths	Delivrs.	Deaths	Delivrs.	Deaths	Delivrs.	Deaths	Delivrs.	Deaths	Delivrs.	Deaths	Delivrs.	Deaths		Delivrs.	Deaths	
Nov.	{ Primiparae Deliveries Deaths	1 9 -	- 8 -	5 8 -	- 17 -	3 1 -	- 4 -	2 1 -	- 9 -	1 9 -	1 Z -	3 10 -	- 11 -	2 11 -	- 11 -	5 11 -	- 10 -	3 10 -	- 89 -	- 2 -	- 1 -	25	1	1	
Dec.	{ Primiparae Deliveries Deaths	5 9 -	1 1 -	2 6 -	- 9 -	3 9 -	- 9 -	1 9 -	- 3 -	4 3 -	- 14 -	7 14 -	- 15 -	4 13 -	- 13 -	4 11 -	- 11 -	2 98 -	- 2 -	- 1 -	32	1			
Jan.	{ Primiparae Deliveries Deaths	3 13 -	- 14 -	3 14 -	- 16 -	5 16 -	- 14 -	6 14 -	- 13 -	6 13 -	- 8 -	3 8 -	1 1 -	- 7 -	4 7 -	- 12 -	5 12 -	- 97 -	- 2 -	- -	55	1			
Feb.	{ Primiparae Deliveries Deaths	2 8 -	1 1 -	6 10 -	- 8 -	6 8 -	- 9 -	2 9 -	- 3 -	3 8 -	- 13 -	4 13 -	1 17 -	1 13 -	6 13 -	- 5 -	5 15 -	- 101 -	- 3 -	- -	41	3			
Mar.	{ Primiparae Deliveries Deaths	2 13 -	- 14 -	6 14 -	- 1 -	3 13 -	- 16 -	5 16 -	- 11 -	4 11 -	- 8 -	1 8 -	1 10 -	- 9 -	5 9 -	- 4 -	4 9 -	- 103 -	- 1 -	- -	31	-			
April	{ Primiparae Deliveries Deaths	2 12 -	- 16 -	7 16 -	- 14 -	4 14 -	- 13 -	7 13 -	- 16 -	4 16 -	1 Z -	9 16 -	- 14 -	6 4 -	- 4 -	1 4 -	- 108 -	- 1 -	- 1 -	42	1				
May	{ Primiparae Deliveries Deaths	2 9 -	- 8 -	3 8 -	- 10 -	4 10 -	- 7 -	5 7 -	- 8 -	2 8 -	- 12 -	4 12 -	1 Z -	4 13 -	2 13 -	- 6 -	6 13 -	- 93 -	- 2 -	- 1 -	32	1	1		
June	{ Primiparae Deliveries Deaths	5 11 -	- 12 -	4 12 -	- 15 -	9 15 -	- 11 -	2 12 -	- 12 -	4 12 -	- 13 -	5 13 -	- 12 -	4 10 -	- 9 -	4 9 -	1 105 -	- 1 -	- -	41	1				
July	{ Primiparae Deliveries Deaths	4 11 -	- 11 -	5 11 -	1 Z -	3 10 -	- 10 -	3 10 -	- 8 -	4 8 -	- 6 -	1 6 -	- 7 -	3 11 -	- 13 -	3 13 -	- 87 -	- 1 -	- 1 -	29	1				
Aug.	{ Primiparae Deliveries Deaths	5 13 -	1 Z -	5 12 -	1 Z -	5 13 -	- 11 -	4 11 -	- 11 -	5 11 -	- 11 -	2 11 -	- 14 -	6 12 -	- 12 -	3 13 -	- 13 -	2 110 -	- 2 -	- 2 -	37	2			
Sep.	{ Primiparae Deliveries Deaths	4 8 -	- 8 -	3 8 -	- 10 -	4 10 -	- 11 -	7 12 -	- 12 -	8 12 -	- 16 -	3 16 -	- 12 -	9 8 -	- 10 -	4 10 -	- 95 -	- -	- -	47	-				
Oct.	{ Primiparae Deliveries Deaths	3 10 -	- 11 -	4 11 -	- 11 -	6 11 -	- 11 -	2 11 -	1 8 -	2 8 -	1 1 -	4 7 -	- 8 -	3 8 -	- 8 -	2 11 -	- 85 -	- 3 -	- -	30	2				
Nov.	{ Primiparae Deliveries Deaths	- -	- -	- -	- 1 -	- 1 -	- 1 -	- 1 -	- 5 -	- 3 -	- 1 -	1 3 -	- 4 -	1 4 -	- 4 -	- 4 -	- 22 -	- -	- -	4	-				
Total Deliveries in each Ward		126	-	129	-	146	-	127	-	134	-	136	-	138	-	123	-	133	-	1123	-				
Total No. of Deaths in each Ward		-	3	-	3	-	1	-	1	-	3	-	4	-	3	-	-	2	-	20	-				
Total No. of Deaths from Zymotic diseases in each Ward		-	1	-	2	-	-	-	-	-	2	-	1	-	-	-	-	-	-	-	6				
No. of Primiparae Deliveries		38	-	53	-	55	-	46	-	48	-	47	-	50	-	44	-	45	-	-	-	426	-		
Deaths in Primiparae from all causes		-	3	-	2	-	-	-	1	-	3	-	3	-	1	-	-	1	-	-	-	-	14	-	
Total No. of Deaths from Zymotic diseases in each Ward		-	1	-	2	-	-	-	-	-	2	-	1	-	-	-	-	-	-	-	-	-	-		

In No. 2 ward, 129 patients were delivered, 53 of which were primiparæ; 2 deaths occurred, both in primiparæ, and both from zymotic causes, but in separate rooms, one, being a private patient, was in the small ward off the large one, and took place on July 5th; the other in the large ward on 26th August; 4 sets of patients having been delivered since the first case, all of whom went out well

In No. 3 ward, 146 deliveries occurred, 55 of which were primiparæ, and all of which recovered but one, a pluripara, from accidental cause, on referring to Table No. 1, will be found ruptured uterus.

In No. 4 ward there were 128 deliveries, 46 being primiparæ, and 1 death, a primipara, accidental, in the month of October, which, on looking at Table No. 1, to be from gastro-enteritis, which she had been suffering from on admission.

In No. 5 ward, 134 patients were delivered, 48 being primiparæ; there were 3 deaths, 1 accidental, a primipara, from gastro-enteritis, which she was suffering from prior to her delivery, and 2 of zymotic causes, 1, a primipara, on November 8th, from pyæmia, and 1 in April, also a primipara, from peritonitis, admitted in a feverish state; her mind being uneasy, some suspicion of her not being married. During the interval between the two cases, 66 patients were delivered in the same ward, all of whom went out well; and on 14 of these occasions the same bed was occupied, and all made a good convalescence.

In No. 6 ward, there were 136 deliveries, 47 of which were primiparæ; there were 4 deaths, 3 being accidental, 2 of which were primiparæ, 1 died of renal disease with gangrene of the uterus, 1 of laryngitis with bronchitis; the 3rd, her third pregnancy, died of purpura hæmorrhagica, which she had been suffering from for some days prior to admission; and the 4th, also a primipara, admitted labouring under acute bronchitis, upon which peritonitis supervened.

In No. 7 ward, 138 patients were delivered, 50 of which were primiparæ, and 3 deaths, 1 of which was primipara, all being accidental, viz., 1 placenta prævia, 1 convulsions, and 1 ruptured uteri.

In No. 8 ward, 123 women were delivered, 44 of whom were primiparæ, and all recovered.

In No. 12 ward, 133 were delivered, 45 of whom were primiparæ, and 2 deaths, 1, a primipara, innupta of mental distress, and 1, also a primipara, of uræmia.

Now, from a careful perusal of the foregoing table it will be clearly seen that where deaths did occur from a zymotic cause, in no one instance did the contagion affect others, and when we take into consideration the circumstances under which these deaths took place, it must be allowed that the results would have been the same, even though their confinements had taken place in any other locality than the Lying-in Hospital.

STATE AND CONDITION.

I must now draw your attention to another point, which should be borne in mind when considering the subject I have to deal with, viz., the question whether "a large maternity is a safe asylum for those seeking its advantages." That is, the state or condition in which the individual is when being admitted, for it must be borne in mind, that where a female is suffering from any acute disease, such as pneumonia, bronchitis, pleuritis, &c., or the mind afflicted with anxiety or despondency, she is extremely liable during the time of her confinement to be attacked with puerperal fever, or peritonitis, from which, together with her previous complaint, she is very likely to fall a victim.

I do not hesitate, therefore, to lay before you a list of the various complaints, and the number of patients suffering from each of them at the time of their admission, as another proof that the hospital is not to be charged with possessing these noxious elements that we have been told prevail in all large maternities.

There were 29 cases admitted in a state of great mental distress, from seduction, &c.; 28 suffering from acute pulmonary complaints; 3 labouring under the effects of beatings from their husbands; 3 with acute gastritis; 6 in convulsions; 9 with variola; 16 in extreme delicacy of health; 14 suffering from hæmorrhage prior to delivery; 1 with purpura hæmorrhagica; 10 where the children had been born on their way to hospital; 3 with intense ascites, and œdema of lower extremities; 5 with syphilis; 1 with ruptured uterus; 1 with abscess in the groin; 2 uterine neuralgia; 1 fibroid tumour of uterus; 1 with severe hæmatemesis.

The next table, No. 3, which I lay before you, is divided, as you may perceive, according to the Poor Law arrangement, into districts, viz., 1, 2, and 3 on the north side of the city, and Nos. 1, 2, 3, and 4 on the south side, and shows in the first column of the respective districts the amount of deaths from zymotic causes, according to the Registrar-General's report, which took place in that district in each month; in the second column the number of deliveries which occurred in the hospital of those admitted from that district; and in the third column the number of deaths which took place amongst them.

Thus, in November, 1871, you find that 27 deaths occurred in district No. 1, north side of the city; that 36 patients were admitted and delivered, who came from the above district, all of whom recovered.

8 deaths took place in No. 2 district, north side, in the same month; there were 20 admitted and delivered from that district, and all recovered.

45 deaths took place in No. 3 district, north side, in the same month; 17 were admitted and delivered from that district, all of whom recovered.

TABLE NO. 3.—Showing the number of Deaths from Zynotic Diseases which occurred in each of the Poor Law Districts during the last twelve months, the number of Patients admitted from each of the above Districts and delivered in the Hospital, and the number of Deaths that took place amongst them.

	NORTH CITY DISTRICT						SOUTH CITY DISTRICT														
	No. 1			No. 2			No. 3			No. 1			No. 2			No. 3			No. 4		
	Deaths in District	Delivered in Hospital	Deaths in Hospital	Deaths in District	Delivered in Hospital	Deaths in Hospital	Deaths in District	Delivered in Hospital	Deaths in Hospital	Deaths in District	Delivered in Hospital	Deaths in Hospital	Deaths in District	Delivered in Hospital	Deaths in Hospital	Deaths in District	Delivered in Hospital	Deaths in Hospital	Deaths in District	Delivered in Hospital	Deaths in Hospital
From Nov. 6, 1871, to Dec. 9, inclusive, to Jan. 6, 1872, - to February 10, - to March 9, - to April 13, - to May 11, - to June 8, - to July 13, - to August 10, - to September 7, - to October 12, - to November 5, -	27 29 51 47 87 72 54 62 17 9 28 6	36 24 25 22 24 23 30 19 29 25 27 21	- - - - - 1 - - - - - -	8 11 14 16 20 20 22 29 12 5 12 7	20 25 29 29 29 19 17 23 20 26 13	- - - - - - - - - - - -	45 38 45 25 38 33 28 42 18 18 29 9	17 12 17 11 16 13 11 19 6 9 16 6	- - - - - - - - - - - -	34 49 188 81 116 95 57 50 24 26 28 17	3 4 5 1 5 1 3 8 3 3 1 -	- - - - - - - - - - - -	10 16 25 24 35 29 29 22 13 9 16 9 4	7 11 19 12 17 9 14 13 9 6 11 10	- - - - 1 - - - - - - -	27 30 35 34 41 32 32 35 32 22 26 17	21 9 9 16 12 13 8 9 7 6 9 8	- - - - - - 1 - - - - -	22 31 36 22 66 33 35 45 33 31 47 15	12 8 9 14 21 16 13 14 16 15 11	1 164 416 164 3
Total,	489	305	1	176	269	-	368	163	-	715	37	-	208	138	1	363	127	1	416	164	3

34 deaths occurred in No. 1 district, south side, from which 3 were admitted, and delivered, and recovered.

10 deaths are registered as occurring in No. 2 district, south side, from which 7 were admitted, and recovered.

27 deaths occurred in No. 3 district, south side; 21 were admitted from same district, and delivered, and all recovered.

22 deaths took place in No. 4 district, south side; there were 12 admitted from same district, and delivered, out of which there was one death of a zymotic nature, but as it would occupy too much of your time to enter into the details of each month, I will merely refer you to the sum total for the year.

Thus, in No. 1, north city district, 189 deaths took place from zymotic causes. 305 patients were delivered who came from that district, out of which we had one death.

In No. 2 district, north city, 176 deaths took place from zymotic causes; there were 269 patients who came from that district delivered in the hospital, all of whom recovered.

In No. 3 district, north city, 368 deaths occurred from zymotic causes; there were 153 patients who came from that district, delivered in the hospital, all of whom recovered.

In No. 1 district, south city, 715 deaths occurred from zymotic diseases. We had 37 deliveries in women who came from that district, all of whom recovered.

In No. 2 district, south city, 208 deaths took place from zymotic causes. 138 patients were delivered in the hospital who came from that district, one of whom died.

In No. 3 district 363 deaths from zymotic diseases occurred. 127 coming from that district were delivered in the hospital, one of whom died.

In No. 4 district 416 deaths took place from zymotic causes. 164 cases were admitted and delivered from that district, among which we had three deaths from zymotic diseases.

Thus showing, that although epidemics prevail with a fatal issue, and that to a fearful extent, outside, and notwithstanding that patients are admitted from the infected localities, they do not exist in the hospital, and if perchance a patient is admitted with disease upon her, it is not disseminated through the institution, never having extended beyond the individual case, as is shown in Tables Nos. 1 and 2, and thus affording a strong proof that the Rotunda Hospital is not the medium of either engendering or spreading disease, and amongst the others I have brought forward, a convincing one, that the mortality (when the various casualties we are exposed to, are taken into consideration) is not as great as that which takes place outside. That, in fact, the institution is fully as safe, if not safer, than their own homes, or isolated dwellings; besides which, it exemplifies how widely our usefulness is extended:

that it is not limited to the immediate neighbourhood of the hospital, but that it spreads its sheltering arms to individuals from all quarters of the city, more than two-thirds of those delivered within the past year having come from the south side, not to mention the number who were admitted from the country and elsewhere.

Having thus endeavoured to give you a clear account of all the occurrences which took place during the past year, without making any further comment of my own, I leave you to draw your conclusion as to whether the hospital may be considered a safe asylum for those seeking its advantages.

The VICE-PRESIDENT said the report which Dr. Johnston had that night, for the fourth year in succession, laid before the Society, probably even exceeded in value his former ones; but indeed, the four reports taken together were perhaps the most valuable contribution that had been laid before the Society during the whole of that period. The great labour which Dr. Johnston had undergone in compiling these reports, their fulness, and their undoubted accuracy and truthfulness, entitled him not only to the thanks of the Society, but to the thanks of all who took an interest in obstetrical matters. Dr. Johnston had protested against a comparison being made between a great Lying-in Hospital and the results of private practice or small Maternity Hospitals. It was needless to add anything to what he had said on that subject. The deductions he had drawn from the facts in the report were self-evident. He had given his reason for presenting these reports—namely, to refute the unjust accusation made against his hospital, and he had proved that a Lying-in Hospital, conducted as his was, was not more liable to mortality than smaller institutions of a similar kind, and he had also shown that even infectious diseases when introduced into it did not necessarily spread. (One thing in the report which struck him was the large proportion of forceps cases; but Dr. Johnston had proved that it was perfectly right to use them frequently, and that the mortality decreased proportionately. One of the tables he presented was very remarkable, and was one that did not exist anywhere else. In it he recorded thirty-five cases in which the forceps was used before the os uteri was fully dilated. They were taught in former days that the forceps should not be used until the os uteri was fully dilated, and they could feel the ear. For himself, he must say he had never felt the ear, but there was certainly a prejudice against using the instrument before the os uteri was fully dilated. Dr. Johnston, however, showed that the forceps could be used with safety and benefit before the perfect dilatation of the os. The 131 cases of chloroform would be looked on with interest, now that the question as to the relative merits of ether and chloroform had been raised. In those cases Dr. Johnston had no hæmorrhage, but

then he had given ergot to prevent it. His (the Vice-President's) experience was that chloroform predisposed to hæmorrhage unless previous precautions were taken.

DR. MORGAN said Dr. Johnston had laid considerable stress on the subject of seduction and the mental anxiety that might be supposed to ensue producing an unfavourable effect as regarded the recovery of the patient. Now, he should be glad to know if these cases were tabulated, and if there was any difference between a well-to-do girl who was confined of her first child and a woman who had been on the town for some time. There was an important distinction to be made between them. In the Lock Hospital, with which he was connected, there was a small maternity, and women were delivered there. He did not think they suffered great mental anxiety on account of their delivery. These, however, were women who would not come under the head of seduction. He must confess, as to the mental anxiety consequent on becoming the victim of seduction, there was one thing against it, namely, that they never saw one of these victims reform. In the Lock Hospital, although every effort had been made to reform them, there had not been a single case of reformation within his experience. He thought there should be great accuracy in these matters, and that the circumstances and history of the individuals who suffered from mental anxiety ought to be stated. He recollected only two cases of women confined dying in the Lock Hospital, and these were cases of puerperal fever.

DR. DENHAM could not allow this valuable report to pass without bearing testimony to its great importance. There were one or two points he felt a little difficulty about—at least they were novel to him. One of these was the application of the forceps before the full dilatation of the os. It was so novel to him and so startling, that he looked on the table stating these cases as the most valuable in the report. At the same time it was a practice that must be undertaken with great caution by junior practitioners. They all knew the great unwillingness there was to attempt to introduce the hand in cases of placenta prævia and convulsions before the os was dilatable if not fully dilated, and they all knew there were numerous cases where the attempt to introduce the hand had been followed by a fatal result—the rupture of the uterus. This was an important subject which he trusted would be fully discussed. Another point was with regard to the crotchet. He did not think Dr. Johnston had given the number of cases where the forceps had failed, and he had had recourse to the crotchet. With reference to the remarks of Dr. Morgan, he did not think they contradicted the conclusion come to by Dr. Johnston. He (Dr. Denham), believed the number of cases of labour that went into a Lock Hospital was very

small indeed. From his own experience, he thought the coming in of a common prostitute to the Rotunda Hospital was a case of extreme rarity, and, considering their state of health, they made very good recoveries. It must be obvious that the case of a young girl coming up from the country, and the victim of seduction, was different from that of an ordinary prostitute.

DR. KIDD felt that at that late hour it would not be right to enter into any very extended observations. He thought Dr. Johnston had conferred a great benefit, not alone on that Society, but on the Dublin School of Midwifery, by these annual reports. He had also conferred a benefit on humanity at large by the manner in which he had conducted the Rotunda Hospital. He had, on many occasions, kindly allowed him (Dr. Kidd) an opportunity of going through that hospital, and to any one who had done so, there could be no surprise at endemic disease not extending in that institution. It would be a happy thing if all private houses were kept in the same state of cleanliness and good ventilation. There were one or two points which he wished to allude to briefly; but, indeed, the paper was so full of important matter, that he felt unable to grasp all that was presented in it; it was a report that a man should take down and study before he should presume to speak upon it. As to the use of the forceps before the os uteri was fully dilated, it was an exceedingly important question. He had, himself, saved lives by the adoption of that practice. It was contrary to the rules laid down in their systematic treatises; but if they met with a case of convulsions (and several of the cases where it was used were of this nature), with the convulsions going on repeatedly and the patient gradually getting worse and worse, and effusion taking place into the bronchial tubes, were they to allow the patient to die. He thought the practice was a good one, and, in judicious hands, could be resorted to without any injury to the woman; and, for his own part, he had made up his mind that in many cases it would be better to incise the os than to allow the patient to go on in that state of extreme hazard. It had been done by others with perfect safety and great advantage. Dr. Johnston spoke of having used the long forceps. He did not know exactly what forceps he meant by that, but in a case he had recently of convulsions, where he had to deliver the patient before the os was dilated, he delivered her with Beatty's forceps, and, in his opinion, that was long enough to deliver any woman, who ought to be delivered by the forceps. There was but one point in Dr. Johnston's paper in which an improvement might be suggested, but whether it would be practicable to carry it out or not he did not know. He referred to Table No. 3. There were similar tables in the former reports, and he had never been able to satisfy himself of their value. The more he thought of it, the more he doubted whether

they were getting accurate and trustworthy conclusions from the facts that were stated in that table. It seemed to him that if they had the population of each district from which the patients came, and if they knew the ratio of zymotic diseases occurring in that population, and then, if they compared the number of cases of zymotic disease which occurred in hospital with the number of patients coming from that district, and if they found that the proportion of the cases of disease was very much less in the hospital than in the district, then Dr. Johnston's proposition would be proved. But at present he did not see that they had any proof of the proposition which Dr. Johnston had brought forward. He was sure Dr. Johnston would give him credit for desiring, by directing attention to this point, that a paper already so valuable might be made of still greater value.

DR. J. W. MOORE said among the many valuable statistics brought under their notice by Dr. Johnston, none were more valuable than those bearing on the sanitary condition of the Rotunda Lying-in Hospital. Last year's mortality in Dublin was exceedingly high, the rate of mortality per thousand persons not being less than 28, whilst the rate of mortality in the Rotunda Hospital during that period was only 17 per thousand—a clear gain of 11. No doubt the inmates of that institution were of the most favourable ages between 18 and 40, but considering their condition as lying-in women, they might throw the one against the other. Looking at the question from another point of view with regard to zymotic diseases, the results were even more striking. In the seven city districts the number of cases of zymotic disease during the last year was 2,935, in a population of 240,000; those in the Rotunda Hospital during the same time amounted to 6. By a very simple rule of three equation, it would be found that the number of cases of zymotic disease that should have occurred in the hospital was 14. Therefore less than half the number in proportion to the number of inmates, as compared with the population of the city districts, occurred in that institution.

DR. DARBY said his experience confirmed that of Dr. Johnston as to the non-spreading of zymotic diseases in lying-in hospitals. With regard to the introduction of the forceps before the os uteri was fully dilated, he might mention a case that occurred to himself. He had been engaged to attend a lady, but happening to be called in to a case of scarlet fever at the time, she would not let him go near her. He recommended a friend to take charge of the case. The lady got convulsions, and the late Dr. Montgomery was sent for. That gentleman had not a forceps with him, and they were obliged to send for him (Dr. Darby). The os uteri was not larger than a five-shilling piece. He delivered the woman with Beatty's forceps, and no case ever did better.

DR. JOHNSON briefly replied. Dr. Athill alluded to his using the forceps in the first stage. His reasons for employing it at so early a period of labour was in consequence of a case in which a fatal result happened one night when he was attending a meeting of that Society. It was a case of third pregnancy. The waters had escaped early. He left the woman with the os not more than the size of a penny piece. He ordered a warm bath to be given her, and on his return, immediately after the meeting, he found the os was not completely expanded, but still the symptoms were such as to induce him to deliver with the forceps. She went on favourably until the third day, when, on going round the wards, he found her in a state of collapse, and she passed away. On a *post-mortem* examination there was found to be perfect separation of the body of the uterus from the cervix. That induced him to adopt the practice he had followed ever since, and he found it had been attended with the greatest advantage, both as to the safety of the mother and the child. It was a course of proceeding, however, which should only be adopted by a careful and experienced practitioner. The operator should be well acquainted with the practice of midwifery, and especially with instrumental midwifery, before he attempted it; but when done with caution and skill, it would be found to be of the greatest use. In one case where death occurred from peritonitis, there were no uterine symptoms whatever. He delivered her before the os was fully dilated; but when he found that by passing his finger in he could expand the os, he did not hesitate to employ the forceps. As to Dr. Morgan's remarks on seduction, it was a notorious fact that whenever they found feverish symptoms occurring after delivery, they constantly traced them to mental anxiety, caused either by seduction or by the husband having deserted or beaten his wife. The only two critical cases he had in the hospital at present were of that kind. In one the husband had deserted his wife, and in the other instance the husband had beaten her. One case of seduction they had was that of a modest girl who came up from the country, fully expecting to die, and who had made all her arrangements for that event, and her father had come up to town to see about burying her. The cases that came under Dr. Morgan's care were altogether different from those in the Rotunda Hospital. The creatures that came to the latter fly from their family and their friends to avoid the shame and scandal that would attach to them. Dr. Denham had referred to the crotchet. He had mentioned five cases in which he employed craniotomy, but in all those cases he applied the forceps first, and failing that, he had to deliver by perforation, and in three cases he used the cephalotribe. He had now used the forceps close on 400 times since he came into the hospital. He was at first prejudiced in favour of Beatty's straight forceps, but in many cases he found when the head was very high up this forceps slipped, and he

now made use of Barnes' double-curved forceps, and in no case did it fail. He had a case last year of a woman with her ninth child. The head never entered the brim. He applied the forceps, and after three-quarters of an hour he brought the child down. It was alive, and on the left *parietal bone* there was a depression of three inches and a half by an inch and a half. He never thought the child would recover, but the child and the mother went out of the hospital quite well. He never would have succeeded in that case with the straight forceps, and it was in those cases where the head was high that he used the double-curved forceps. As to Table No. 3, his reason for giving it was that in the attacks made against large maternities it was always said that whenever an epidemic prevailed in Dublin it was sure to commence in the Rotunda Hospital, and he wanted to show that while contagious maladies existed outside in the different districts from which the patients came, yet there was no sickness in the house. Patients constantly came from infected districts, but they did not convey the disease to others in the Institution. The table might not be of much positive value, but he thought it was of some comparative value.

The Society adjourned.

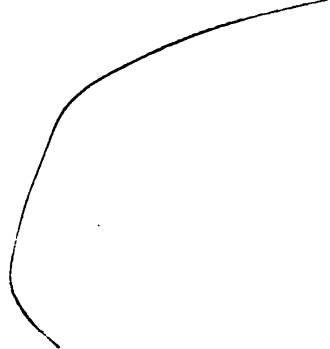


Fig. 1.



Fig. 2.

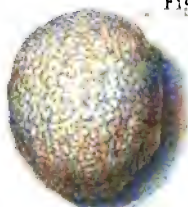


Fig. 3.

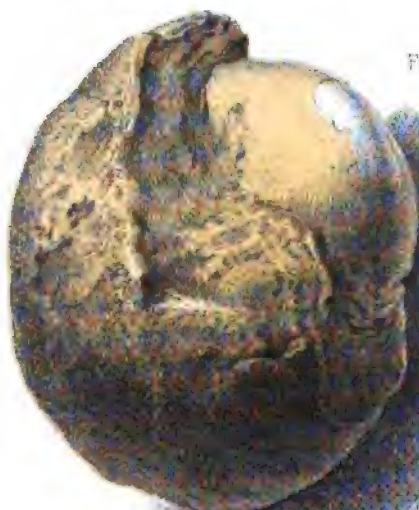


Fig. 4.



Fig. 5.



THE DUBLIN JOURNAL

OF

MEDICAL SCIENCE.

MARCH 1, 1873.

PART I.

ORIGINAL COMMUNICATIONS.

ART. V.—*Contributions to Operative and Clinical Surgery—Lithotomy and Lithotrity—illustrated by six cases.* By HENRY GRAY CROLY, F.R.C.S.I., Licentiate of the King and Queen's College of Physicians; Surgeon to the City of Dublin Hospital; Senior Demonstrator of Surgical and Descriptive Anatomy, Royal College of Surgeons; Member of the Council of the Surgical Society of Ireland, &c., &c.

I.—LITHIC ACID CALCULUS, WITH OXALATE OF LIME NUCLEUS, REMOVED BY LATERAL LITHOTOMY.

II.—OXALATE OF LIME CALCULUS, REMOVED BY LATERAL LITHOTOMY.

III.—PHOSPHATIC CALCULUS, REMOVED BY LATERAL LITHOTOMY.

IV.—PHOSPHATIC CALCULUS OF ENORMOUS SIZE, WITH A SMALLER STONE, REMOVED BY LATERAL LITHOTOMY.

V.—PHOSPHATIC CALCULI; OPERATION DECLINED.

VI.—PHOSPHATIC CALCULUS, REMOVED BY LITHOTRITY.

URINARY calculus being one of the most painful and fatal maladies that can afflict the human frame, the attention of the profession has been naturally attracted to the disease from the earliest days, with the hope of affording relief to the sufferer, either by the resources of medicine, or by the art and science of surgery.

Hippocrates graphically describes the symptoms of calculus in the kidney and bladder; and the recent great public event, which has so startled the world, has imparted a fresh and almost romantic interest and importance to the disease. The sufferings of the

ex-Emperor of the French—the two operations resorted to for his relief, and the unhappy termination, have made the public mind but too painfully familiar with the details of a malady previously but little known to any but the surgical profession. The public at large are now made aware, even through the non-medical press, that for the cure or relief of this disease the resources of art are limited to two—either making an incision through which the calculus may be extracted, or (as in the instance of the illustrious patient at Chiselhurst) by another even more delicate operation, the crushing of the concretion into fragments, so that they may be easily expelled “*per vias naturales*.”

While the ex-Emperor's malady is the all-absorbing topic in every circle, professional and non-professional, the present is a peculiarly opportune time to report cases of the disease as they occur in practice, with the minutiae and result of operative interference, whether by the old operation of lithotomy, or the modern expedient of lithotrity.

The profession were not surprised at the issue of Napoleon's illness. In him the energies of the vital principle were much depressed and almost exhausted, by long-continued mental and physical suffering, which, at his period of life, could ill be borne, and hence the rapid sinking and sudden cessation of the heart's action; and the *post-mortem* examination revealed a large amount of renal disease that had existed for some time, “though not suspected,” which, had it (or could it have) been detected during life, would have rendered any operation unpromising.

Stone in the bladder is, from some unexplained cause, a comparatively rare disease in this island, especially in contrast with England and Scotland. It has fallen to my lot during the last four years to have had under my care, in the City of Dublin Hospital, six cases of urinary calculi, in four of which I performed lateral lithotomy, in one lithotrity, and in the sixth case I declined operative interference of any kind, in consequence of advanced kidney disease.

The following is a detail of the several cases, their history, symptoms, operations, subsequent treatment, and results:—

CASE I.—Michael Gavin, aged six years, a fine ruddy-looking boy, was admitted, under my care, into the children's ward of the City of Dublin Hospital, on the 1st of October, 1868, suffering from stone in the bladder.

Previous history.—From infancy he had been considered strong and healthy (confirming the observation which has been often made, that it is amongst the healthiest-looking boys that cases of stone occur). For about three years his parents observed that he experienced some difficulty in passing water, altering his position at the time, and sometimes crying out from severe pain, referred to the extremity of the penis, which he was in the habit of constantly pulling, the flow of urine often stopping suddenly. Having suffered much from these symptoms for several months, during a paroxysm of pain he passed a small stone (about the size of a pea), after which he became much relieved, but in the following month of April the symptoms recurred with increased intensity.

Believing from this history that it was a case of stone in the bladder, I had the boy placed under the influence of chloroform, and having introduced a sound, I readily detected a calculus lying at the neck of the bladder; from the sensation conveyed by the instrument, I considered the stone to be about the size of a marble and hard.

From the time of the child's admission into hospital the nurse observed that he passed water involuntarily during sleep, and suffered intensely when passing it during the day. He was ordered nutritive diet and warm hip-baths—lime-water and milk.

The operation was fixed for November 9th. A teaspoonful of castor-oil was given on the previous night, and early on the morning of operation an enema of tepid water was administered, so as to thoroughly empty the rectum. The child retained the urine for nearly two hours before the operation. The table in the operating theatre was raised to the height of $3\frac{1}{2}$ feet by means of a mattress, folded blankets, and sheet, covered by a "Mackintosh," which was closely turned in. (These arrangements of the height of the table, so important to the operator, and many other necessary details, will be found fully described by Mr. Butcher in his work on Operative Surgery). The child having been placed on the table and chloroformed, the staff was introduced, and the stone struck again. He was secured in the lithotomy position by means of tapes; his hips were brought closely to the end of the table, and an assistant at each side held his thighs apart and fixed his feet flatly and firmly on the table. The staff, *grooved on its side*, entrusted to a third assistant, was held closely up under the pubes, so as to elevate the urethra from the rectum.

The following instruments and appliances were arranged on a

tray beside me:—a long-handled scalpel with short blade, a probe-pointed knife, blunt gorget, forceps, *canule à chemise* (with tapes), long-handled tenaculum, ligatures, sponges, and charpie; and a basin of hot water (to warm the instruments), and several napkins were also at hand.

Before beginning the operation I introduced the *second* finger of my left hand into the rectum, to excite contraction of the bowel and make sure of its being empty. The integuments of the perinæum having been stretched by my left forefinger and thumb, I commenced the operation by making an incision from an inch in front of the anus and a couple of lines to the left of the raphe, downwards and outwards, terminating at a point nearly midway between the anus and tuberosity of the ischium, but a little closer to the latter; this incision divided the superficial structures. The second incision, commenced a little lower down than the first, cut through the deep tissues. I then introduced the forefinger of my left hand, and felt for the membranous portion of the urethra, which I pressed with my finger-nail into the groove of the staff; the knife (still in the wound and guided by the finger nail) was next inserted into the staff, and moved from side to side, to ensure its being *in the groove and not on its edge*. My hand was now depressed, and the knife, held like a pen, glided along the staff (the edge of the knife being directed laterally and its back kept up into the groove) until it reached the stop at the end of the staff, when the urine trickled out. I again introduced my left index-finger, guided by the staff; one turn of my finger enabled me to pass it freely into the bladder, when the stone was immediately felt. The urine now flowed freely. The sphincter of the neck of the bladder grasped my finger like an Indian-rubber ring. I next passed in the blunt gorget, and at this step of the operation I requested my assistant to withdraw the staff; the small forceps, previously warmed and oiled, directed by the gorget, was then passed into the bladder. The gorget being withdrawn, the stone was easily grasped in the forceps and removed. It was the size and shape of a large peach stone; on section showing a hard nucleus, composed of oxalate of lime, the exterior being lithic acid (*vide* Plate X., calculus Fig. 1). The wound was sponged, and the *canule à chemise* introduced, secured by tapes, and moderately plugged with charpie to keep it steady; the legs were untied and the knees bound together. The child, wrapped in a blanket, was carried to his ward and placed in a bed arranged with a "draw-sheet" and "Mackintosh," and a hot jar

to the feet. The resident pupil was directed to remain at the child's bedside, the foot of the bed being turned towards the window, to admit of a thorough view of the incision should any hæmorrhage occur. Patient visited in three hours; no hæmorrhage; stomach very irritable from the chloroform; urine not passing sufficiently through the tube, which seems to act as a foreign body. As there was no tendency to bleeding I removed it; the urine then flowed freely on the sheet. Patient was ordered ice, milk, and chicken-broth.

11 p.m.—Pulse 140. Urine flowing on cross-sheet, which has been occasionally drawn to keep the hips dry. Patient was restless since operation; takes the ice and beef-tea; stomach not yet settled.

10th.—Child slept some hours; urine passed freely. Evening visit.—Pulse 130. Passed a quiet day; slept occasionally.

11th.—Patient slept well; countenance cheerful; complains of pain in the wound; urine continues to flow; buttocks reddened by the irritation of the urine; ordered to be dusted with flour; bowels moved naturally.

12th.—Urine flowing.

13th.—Slept well; sheet constantly wet; wound granulating.

14th.—Countenance very good, of ruddy colour; urine passing.

15th.—To-day, for the first time, urine came through urethra.

16th.—Urine passing both by urethra and wound; incision granulating.

17th.—Still progressing favourably.

18th.—Almost all the urine coming through urethra.

24th.—Wound healed.

December 1st.—Allowed to sit up.

6th.—Discharged quite well.

My former apprentices, Drs. Burney and Cox, were most attentive to this patient, and took the daily notes of the case.

CASE II.—William Hogan, aged five years, a nice-looking, fair-complexioned boy, was admitted, under my care, into the children's ward of the City of Dublin Hospital, on November 24th, 1869.

Previous history.—The mother, who brought the child to me for advice, states that for the past two years she observed him forcing unnaturally when passing water, and pulling at the end of his prepuce; she frequently checked him, thinking it During the last six months his sufferings were great, (

when the bladder was empty; the rectum became prolapsed, and bled occasionally. The child lost his appetite, and became very thin.

My friend and pupil, Mr. (now Doctor) Charles Allen, chloroformed the child in my study. On introducing the sound, I at once detected the stone, which emitted a "sharp click."

On admission to hospital, I ordered the patient lime-water and milk, nutritious diet, and a warm hip-bath at night. On November 24th I performed lateral lithotomy, carefully following all the steps of the operation as detailed in Case I., the child having retained his urine, for nearly two hours before operation. The urethra was readily cut into, and the finger introduced; only one knife being used in this case also. The stone, which was struck before the operation was commenced, could not now be felt. However, by a sharp tap over the pubes, the calculus was dislodged, and fell against the forceps, but slipped away, as if into a fold of the mucous membrane; it again escaped two or three times before it could be seized and withdrawn. The stone is small and of the mulberry variety, as anticipated by the "sharp click" on sounding (*vide* Plate XI., Fig. 1). There was no hæmorrhage, except from the edges of the incision; the *canule à chemise* was introduced, and the child removed to bed. Ordered four minims of tincture of opium with brandy and water.

12 o'clock noon.—Urine, slightly tinged with blood, coming *guttatim* through canule.

2 p.m.—Child has slept; cross-sheet wet, and less tinged with blood.

2.50 p.m.—Cross-sheet drawn, saturated with urine.

6.30 p.m.—Urine still passing freely. In forty-eight hours after the operation the canule was removed.

Dec. 6th.—Urine passed per urethram.

8th.—Prepuce much swollen; ordered warm hip-bath.

From this date the child made an uninterrupted recovery.

CASE III.—Robert Smith, aged thirty-five years, was admitted, under my care, into the City of Dublin Hospital, on July 13th, 1871.

Patient had been suffering for fifteen years from uneasiness in the bladder, and observed that his urine was high-coloured. He was treated for stricture, and occasionally had instruments passed. In Dec., 1861, when jumping, he felt something fall suddenly

into the bladder, and immediately after suffered intense pain in the perinæum, and passed bloody urine. In July, 1871, he consulted my friend, Dr. Bagot, of Enniskillen, who sounded him, and, having detected a stone, kindly recommended the case to my care.

Condition of patient on admission into Hospital.—Is highly nervous, has an anxious expression, and dreads the idea of any operation; has a feeling of weight in the perinæum (always in same situation), and frequent desire to pass water; no pain at end of penis. Ordered sedative draughts, warm hip-baths, and nutritious diet.

On sounding the patient, I struck a soft stone, which lay directly inside the neck of the bladder, and was fixed in that position. On a subsequent occasion I introduced a lithotrite, and measured the calculus, which was found to be the size of a large walnut. Though the utmost gentleness was used in sounding, it caused rigors and intense pain. The urine contained mucus and blood, but was free from pus or albumen. I decided on performing lithotomy in preference to lithotrity, for reasons which I shall yet explain.

August 1st.—The patient, having retained his urine for a couple of hours, was placed on the operation table, and chloroform administered. Although the chloroform was pure, it took but little effect, and nearly an hour elapsed before there was any insensibility. At length a slight degree of anæsthesia was induced; I then availed myself of the opportunity of having the staff passed, and the patient "tied up." I cut into the bladder without any difficulty, using *only one knife*. My left forefinger, passed along the staff, felt the calculus lying inside the neck of the bladder; the blunt gorget was next introduced (guided by the finger), and lastly the forceps, which seized and extracted the stone. On examination it was found to be broken, and on re-introducing my left index finger into the bladder, I felt the remaining portion *encysted* behind the prostate. I re-introduced the gorget and forceps, and was enabled to extract the second portion. The stone, being soft and encysted, caused its separation into two pieces (*vide* Plate XI., Fig. 2). The bladder was washed out by means of Clover's apparatus, the wound sponged, and a *canule à chemise* introduced. The patient was then unbound and carried to bed; hot jars were applied to the feet, and an opiate draught administered.

1 o'clock p.m.—Urine passed freely through the tube; slight venous oozing from wound. 2 p.m.—Pulse 96; cross-sheet saturated with urine; no hæmorrhage. 3 p.m.—Pulse 100; urine flowing freely. 5 p.m.—Patient in same state. 9 p.m.—Patient has slept. 11 p.m.—Took beef-tea and ice.

August 2nd.—Pulse 98; urine still passing.

3rd, 10 a.m.—Canule removed.

6 o'clock p.m.—I received a message from the resident pupil on duty, stating that the patient was very ill. On arriving at the hospital, I found him perspiring profusely, and learned that he had several severe rigors; the cross-sheet was quite dry, and the bladder distended. I at once placed the patient in a warm bath, and in a short time he passed several clots of blood, after which the urine flowed freely, affording instant relief.

2 a.m.—(Report of Resident Pupil—Mr., now Dr., Sterling, who watched the case with most praiseworthy care).—Patient complains of pain and uneasiness in the bladder; no urine passing; warm bath repeated according to directions.

2.40 a.m.—Sudden gush of urine, bringing away a few large clots; some urine has come also per urethram. Pulse 88; countenance more cheerful.

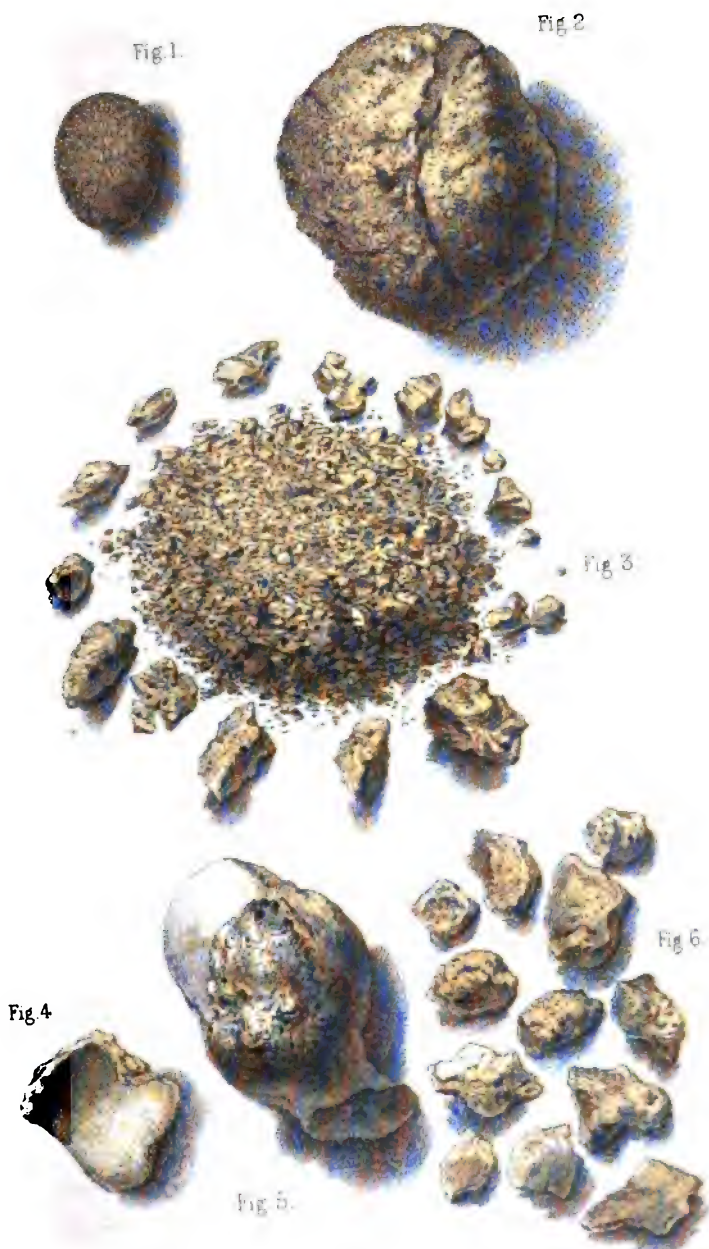
3 a.m. to 5 a.m.—Patient slept; urine passing uninterruptedly; has taken some nourishment.

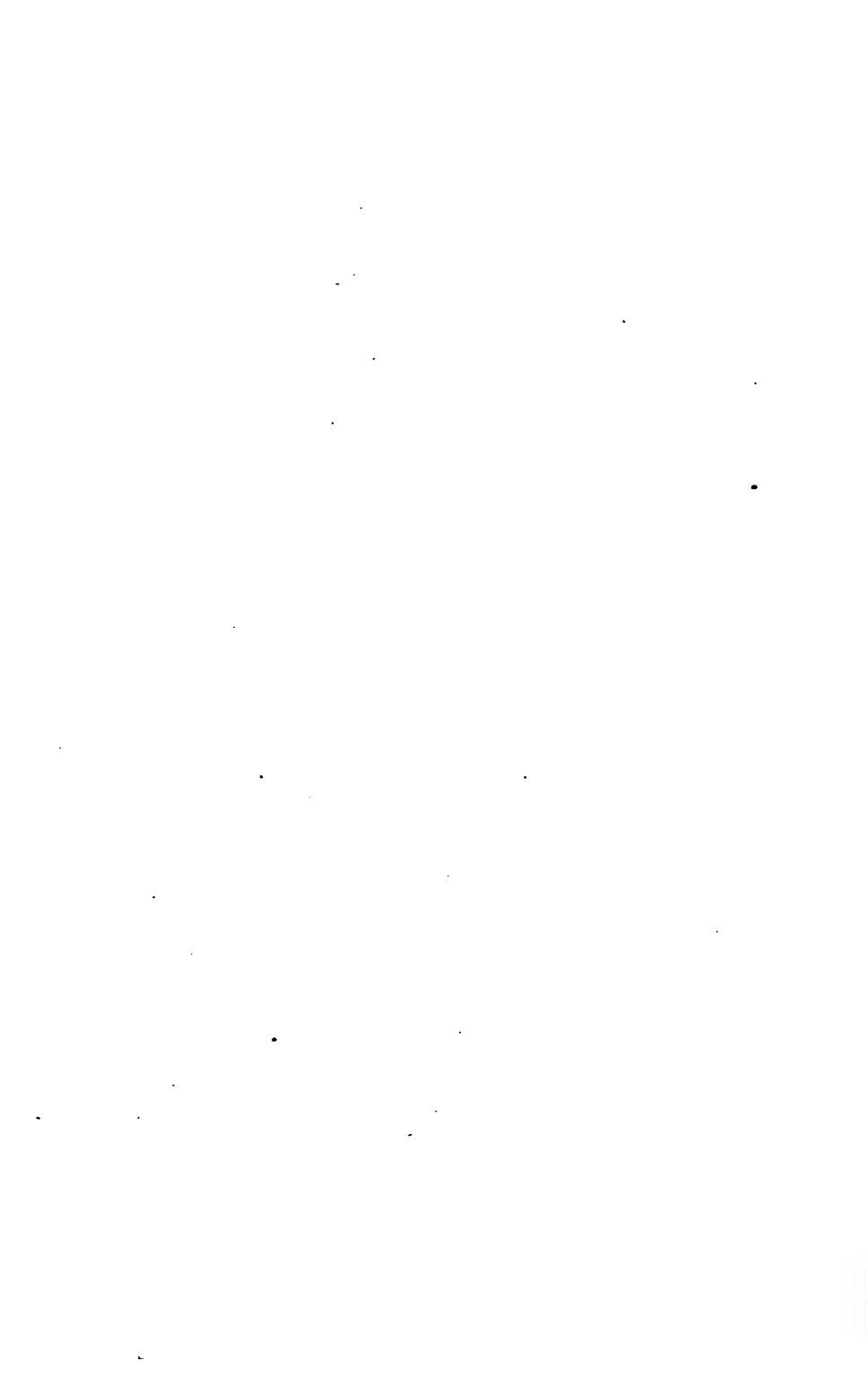
8.15 a.m.—Pulse 80; some clots passed in urine; has taken food.

The wound was now ordered to be frequently washed with a solution of carbolic acid, and dressed with carbolic ointment. The incision gradually healed, and on August 30th the patient was discharged perfectly cured.

CASE IV.—Christopher Campbell, aged forty-eight years, was admitted, under my care, into the City of Dublin Hospital, on the 25th of June, 1872.

Patient states that from boyhood he experienced difficulty in passing water, and that he often cried from the pain. Eighteen months previously to his consulting me the symptoms became still more urgent; he observed blood and a thick heavy sediment in the urine; suffered from pain in the penis and tenesmus. Later the rectum became prolapsed during his efforts to pass water. He consulted several surgeons, and was treated for stricture, piles, and for the prolapsus recti. On hearing the details of his case, I





sounded him, and struck a stone. Ordered warm hip-baths and nutritious diet. The urine on analysis was found to contain mucus and blood, but no trace of albumen. He was ordered decoction of *triticum repens*, and, the urine being alkaline, dilute nitric acid in mixture, with tinct. *hyoscy.* and *buchu*, was also prescribed.

By this treatment the urine became almost natural in colour. I then measured the stone, which was found to be about the size of a walnut. Having decided on the operation most suitable for the case, I performed lithotritry on the 30th June; Weiss's modification of Sir Henry Thompson's was the instrument used. A large quantity of phosphates was passed, and the patient experienced much relief. The urine gradually became clear, and on making another exploration I struck a second stone, which I was unable to seize in the lithotrite in consequence of a contraction of the bladder on the instrument. I accordingly decided on performing lithotomy, as soon as all irritation should have subsided.

July 16th.—Patient having taken a dose of castor oil on the previous night, and the rectum being cleared by an enema administered early in the morning, was placed on the operating table, and chloroform given. Severe spasm of the glottis prevented its being persevered in—the patient having a weak-acting heart, but no murmur.

I cut into the bladder, as in cases already detailed, and removed a large sized stone having a facette (*vide* Plate X., Fig. 4). On again introducing my left forefinger, I felt another calculus of enormous size lying in a pouch behind the prostate; the neck of the cyst almost covered the stone, but admitted the finger. I introduced the probe-pointed knife, and enlarged the neck of the cyst; the stone was seized in a curved forceps, but slipped several times from the grasp of the instrument. The left forefinger was next passed into the rectum, but the calculus could not be dislodged; a scoop was also tried. At length I succeeded in detaching the stone with my finger from the walls of the cyst, in which it was firmly lodged. The calculus weighed nearly *five ounces* (*vide* Plate X., Fig. 3). The bladder was washed out with a strong syringe, the canule introduced, and the knees being bound, the patient was removed to bed. Brandy and water and a full opiate were given.

2.30 p.m.—Pulse 90; urine coming through the tube; clots of blood occasionally blocked up the canule; by syringing with iced-water they were removed. Brandy and iced-water ordered.

9.30 p.m.—Patient has slept; pulse 96; has vomited. Ordered lime-water and milk—ice to be continued.

17th, 1 o'clock a.m.—Patient had a rigor, and vomited greenish-coloured fluid; pulse 102; urine passing through the wound.

10 a.m.—Slept soundly during the night; pulse 104; urine stopped flowing through the canule, but on syringing the tube with water a clot was removed, after which the urine passed freely.

18th.—Pulse 102; urine passes in gushes through the tube; a few drops have come per urethram; ordered chicken-broth instead of beef-tea, the stomach being out of order.

10 p.m.—Pulse 104; patient complains of pain and tenderness over pubes; ordered hot turpentine fomentations and opiate draught.

19th.—Pulse 98; canule removed, as it seemed to cause irritation; wound syringed with tepid water, containing a weak solution of carbolic acid; hips reddened from irritation of urine, which was found to be slightly ammoniacal; half a pint of decoction of *tritimum repens* to be taken three times a day with a few drops of dilute nitric acid.

20th.—Pulse 106; patient passed a quiet night.

10 p.m.—Pulse 110; restless.

21st.—Pulse 120; heart's sounds feeble; patient looking badly; ordered brandy and egg.

22nd.—Pulse 112; patient feels stronger; urine flowing through wound.

23rd.—No change.

24th.—Pulse 98; patient had a chop for dinner.

27th.—Wound touched with nitrate of silver.

29th.—Pulse 100; threatened with syncope.

31st.—Pulse 104; complains of burning sensation in the epigastric region.

August 2nd, 2 o'clock a.m.—The resident pupil was called to see the patient, who had been attacked with syncope. He found him bathed in a cold, clammy perspiration, with cold extremities and a very weak pulse; stimulants were freely administered; hot jars applied to the feet, and mustard over the heart and calves of the legs; a slight improvement then took place.

7.30 a.m.—Symptoms returned; he sank rapidly, and died at 7.45 a.m.

I am indebted to my former apprentice, Mr. (now Dr) James

McCloughry, for the daily notes of this case, which he watched throughout most carefully in his capacity as resident pupil.

Autopsy.—On opening the pelvic cavity, the external surface of the bladder presented an appearance resembling that of the uterus; internally the mucous membrane was found to be extensively diseased and covered with phosphatic concretion. Behind the prostate gland the large pouch was observed in which the enormous stone had been encysted; the coats of the bladder were much thickened from chronic inflammation. No trace of disease could be found in the kidneys, heart, or lungs.

CASE V.—Thomas Killeen, aged eighteen years, was admitted, under my care, into the City of Dublin Hospital, on April 23rd, 1870, suffering from symptoms of stone in the bladder. On inquiry into the history of his case, I ascertained that previously he had been twice operated on by the lateral method, a large stone having been extracted on each occasion.

The patient, on admission, presented a wretchedly emaciated appearance, and suffered from severe pain in the loins, rigors, and perspiration, and had "fits of the stone" of intense severity. His urine was albuminous, and contained pus in large quantities. Sounding the bladder was accomplished with great difficulty, in consequence of stricture of the urethra, resulting from the two former operations. The calculus was measured, and found to be large and soft. It was very difficult to free the stone from the grasp of the instrument.

Palliative treatment was alone adopted in this case: the evidence of such serious complications as albuminuria, with chronic nephritis and pyelitis (the result of long-continued irritation of the bladder and urethra), and the "phosphatic diathesis," fully contra-indicated any further operative interference.

The patient was worn out from pain and hectic symptoms, rapidly sank, and died on the 12th May.

On making a *post-mortem* examination, two phosphatic calculi and several fragments of stone were found (*vide* Plate XI., Figs. 4, 5, and 6), the interior of the bladder was found to be extremely small, and the coats enormously hypertrophied; the mucous membrane was extensively diseased, and the kidneys showed the appearance observed in advanced Bright's disease.

The bladder was presented to the Pathological Museum of the Royal College of Surgeons.

CASE VI.—Philip Cooney, aged sixty, was admitted (on the recommendation of the late Dr. Beatty), under my care, into the City of Dublin Hospital, in October, 1871. He had been suffering for two years from severe pain in passing water, and along the course of the urethra. From time to time he sought medical advice, and obtained temporary relief; about one month before coming under my care he, for the first time, passed blood from the bladder.

Condition on admission to Hospital.—Patient was much debilitated, and there was a strong urinary odour from his clothes: his skin was hot, and he had considerable thirst; there was frequent desire to pass water; the urine was very offensive, and contained ropy mucus and blood; ordered nutritious diet and warm hip-baths, and a mixture consisting of dilute nitric acid, tinctures of buchu and hyoscyamus, with decoction of pariera brava, also buchu tea. Notwithstanding this treatment the symptoms of vesical catarrh continued, and the patient passed blood in enormous quantities, mixed with the urine; gallic acid (in ten-grain doses) was administered three times a day, with the desired effect of checking the hæmorrhage. Subsequently, the bladder was washed out on alternate days (by means of a double catheter) with tepid water containing a small quantity of dilute nitric acid, and the fætor of the urine was corrected by the occasional use of a weak injection of carbolic acid. Under this treatment the patient's general health considerably improved; the vesical irritation subsided, and the urine became clearer. Sounding the bladder was now performed with Sir Henry Thompson's instrument, and I detected a soft stone about the size of a walnut. As the patient's health, although much improved, was not sufficiently restored to admit of operative interference, I advised change of air and repetition of the medicine. The man occasionally called at the hospital to report his condition; and in June he was re-admitted. I then decided on performing lithotrity, a procedure which met the approval of my colleagues. I accordingly operated as follows:—

The patient (who was directed to retain his urine for a couple of hours) was placed on an iron bedstead about two and a half feet in height, with a mattress and hair cushion to slightly elevate the pelvis. Weiss' modification of Sir Henry Thompson's lithotrite was the instrument used. The stone was seized in the centre of the bladder (the area for operation) and crushed. The time occupied in operating, including the introduction of the lithotrite, was between three and four minutes. The patient, though very

sensitive to pain, did not complain of any suffering during the operation, and not a drop of blood was passed; he was removed to bed, which was arranged with hot jars; had a warm drink, and hot flannels were applied to the abdomen and perinæum; he was directed to remain in the horizontal posture, and to pass water in that position for a couple of days, to prevent the fragments of stone falling on the neck of the bladder. Decoction of triticum repens was prescribed; the detritus was passed (*vide* Plate XI., Fig. 3), and the urine for some days remained muddy, but finally became clear.

The operation thus detailed was repeated at three subsequent "sittings," and the patient was discharged cured. He presented himself lately at the hospital in good health, without any trace of his previous ailment.

I may here remark, that the mode of introducing the lithotrite requires some practice, and is not to be conducted in the ordinary method of passing a catheter; for if the instrument be not skilfully introduced the urethra is very liable to be lacerated. The surgeon may stand either at the patient's right side or between his legs. The lithotrite, previously warmed and well oiled, should be introduced parallel with the abdomen; the penis, held between the thumb and forefinger, is to be drawn up on the instrument, which is then to be gradually raised to the perpendicular, when it will glide down by its own weight to the opening in the triangular ligament, and being then depressed between the thighs will enter the bladder. Thompson's sound, being shaped like the lithotrite, requires the same manipulation.

The details of the foregoing six cases of stone in the bladder, which were under my care during the past four years, afford several points of much interest to the surgeon, each case presenting some peculiarity in its progress and treatment.

In Case I., before operation, the bladder was allowed to retain the urine for about a couple of hours, instead of being injected with water, as recommended by some operators. I adopted this practice on the high authority of Sir Henry Thompson, who quotes Cheselden and Crichton as preferring to have the bladder empty; the stone being then found close to the internal incision at the neck of the bladder. The bladder was readily entered, and the stone, which lay immediately inside the neck of the organ, was removed with facility. The *cunule à chemise*, having acted as a

foreign body, was in consequence removed in a few hours. In this and the two subsequent operations I advisedly used *one knife only*, reserving, should occasion arise, the probe-pointed knife, but the necessity for its use did not occur. In Case IV., however, owing to the large size of the stone and its being encysted, I was obliged to use the second knife. The chief difficulty, in my judgment, in lithotomy is getting the knife into the groove of the staff; when so inserted (its edge being properly directed), the knife is passed steadily along the groove (dividing the membranous and prostatic portions of the urethra) until stopped at the end of the staff. The bladder is thus sufficiently opened to allow the removal of a moderate-sized stone; and further I may remark, that if the knife is withdrawn for the purpose of using the probe-pointed instrument, it may not be an easy matter, especially in the child, to re-enter the groove of the staff. A free external and a limited internal incision (provided it be of sufficient extent to allow the extraction of the stone without violence) are points of considerable practical importance.

At one of my lithotomy operations I was favoured with the presence of my friend and former fellow-student, Dr. Purefoy Colles, of the Indian Army, Fellow of our College of Surgeons. He informed me, as the result of his large experience (having operated on a great number of natives by lateral lithotomy), that when it was necessary, in consequence of the size of the stone, to enlarge the neck of the bladder so as to admit more than two fingers, the result, as a rule, was fatal; but when the sphincter vesicæ grasped the finger, or even admitted two fingers, that the case recovered. This remark was corroborated by my friend, Dr. James Kelly, F.R.C.S.I., of the Indian Army, who also has much experience as a lithotomist.

In Case II. the calculus was hard and small, and on some occasions could not be struck with the sound; it passed upwards into a pouch behind the pubes, and I anticipated a difficulty in seizing it with the forceps.

In sounding the bladder in the child, there is usually no difficulty in striking the stone, owing to the shape of the viscus at that period of life; occasionally, however, as in this case, the stone may escape detection by getting into a fold of the mucous lining. On withdrawing the sound from the bladder the calculus is sometimes struck in the *concavity* of the instrument. Thompson's sound having a very short curve, is now a favourite with many

surgeons, and can be used as a catheter to empty or inject the bladder should necessity arise.

The operation in this case, so far as entering the bladder was concerned, was accomplished with facility, but I had some delay in catching the stone. I easily felt it with my forefinger, but it glided into the pouch, and required a tap of the fingers over the pubes to dislodge it; when once grasped in the forceps it was easily withdrawn, in consequence of its small size.

In cases of lithotomy in early life, it is unusual to experience any difficulty in *extracting the stone*; the difficulty lies in getting safely into the bladder. Lithotomy in the child is one of the most successful operations in surgery. It is, however, capable of being rendered one of the most fatal, and all eminent lithotomists admit that it is a very anxious undertaking. Mr. Erichsen remarks:—"When children die after lithotomy, they do not usually perish from the same causes that prove fatal in the adult, viz.—kidney disease and acute pelvic inflammation and infiltrations; but they generally die in consequence of some accidental violence having been inflicted during the operation, such as mistaking the recto-vesical space for the interior of the bladder, tearing across the urethra, and non-extraction of the stone—wounding of the recto-vesical fold of peritoneum or perforation of the bladder by the point of the knife."

The blunt gorget is a most useful instrument in lithotomy of children, being a safe director of the forceps into the bladder. If the stone should not be seized in the forceps at the first attempt, and if its re-introduction be required, then there is danger of the operator (in his anxiety to grasp the stone, and attempting to pass the forceps on his finger *without the gorget*) missing the opening into the bladder; the result of this may be serious injury to the soft parts and failure in extracting the stone.

In Case III., although the calculus was soft, and of a size suitable for lithotrixy, I decided on performing lithotomy, for the following reasons, viz.:—The patient was extremely nervous, and had a very sensitive urethra and irritable bladder, and suffered much on the introduction of the sound. Moreover, I felt convinced that the stone was partly *encysted*, and situated behind the prostate gland; because the patient felt a dull weight in the perinæum, did not complain of pain at the extremity of the penis, and when the calculus was grasped in the lithotrite to ascertain its size, it could not be moved in the bladder. I considered it better surgery to relieve

my patient at once from the foreign body, than to subject him to the risk of rigors and subsequent irritative fever.

The ineffectual attempts to produce anæsthesia by chloroform in this patient were trying to my patient and myself, and wearying to my colleagues, the class, and the visiting surgeons who honoured me with their presence. No difficulty arose in cutting into the bladder, and more than half the stone came away in the forceps owing to its softness, the utmost gentleness having been used in the extraction; the remainder was *encysted*, as anticipated, and removed by means of a scoop. The *canule à chemise* answered admirably in this case; however, on the day of its removal, the patient got complete retention of urine, and was attacked by severe rigors. A warm hip-bath and a full dose of opium acted like a charm. The retention of urine was evidently caused by clots of blood obstructing the neck of the bladder, the result of a slight hæmorrhage following the withdrawal of the canule.

Rigors occurring after the operation of lithotomy are often indicative of serious mischief; while retention of urine, if coming on immediately after the operation, may be caused by hæmorrhage, or by too free a division of the prostate.

Case IV. is interesting in many points of view. The patient had been suffering, as long as he could remember, from symptoms of calculus, and for several years had worked as a labourer, having one enormous stone encysted, and two others free, in the bladder; he was never sounded till he consulted me, but, according to his own statement, was treated for stricture of the urethra and prolapse of the rectum. On the first sounding of this case I was forcibly struck with the contraction of the bladder on the instrument, and on measuring the stone, which was free in the bladder, it could not be moved sufficiently to enable me to ascertain if a second existed. The presence of the enormous stone, encysted, could not have been detected in this case by sounding, before the bladder was opened, as the cyst almost entirely enveloped the stone, leaving merely a small portion free at the orifice. This point of practical importance has been referred to by Sir Henry Thompson as having occurred in his practice. The patient had a very large urethra, and bore the introduction of instruments so well that I decided on giving him the chance of cure by a crushing operation. Lithotrity was performed, and a large quantity of mortar-like sediment was passed, and the patient expressed himself much relieved by the operation. When the urine became clear I attempted a second "sitting," but

then found that behind the prostate there was a very large calculus, which could not be grasped in the lithotrite. The patient had a weak-acting though not actually diseased heart: he was made aware of the necessity for a cutting operation, to which he consented, but felt disappointed at lithotrity not being as successful in his case as it promised to be in case No. VI., the subject of which having occupied an adjoining bed, and had undergone two "sittings" with much relief. At the time of operation chloroform produced severe spasm of the glottis, and the heart's action became feeble. The bladder was easily cut into, and a stone (with a facette) removed. On introducing the left forefinger a second time, I felt another stone of enormous size behind the prostate, the neck of the cyst surrounding the concretion like an Indian-rubber ring. The calculus was seized by the large curved forceps, but was so large, and so firmly fixed, that I decided on enlarging the neck of the cyst with a probe-pointed knife; having done so, the stone was grasped by the forceps, the left forefinger being passed into the rectum to assist in the removal of the calculus, but I could not succeed. After several attempts I was enabled, by means of a scoop, the forceps, and an occasional touch of the left index-finger, to dislodge the large stone. No bad symptoms followed this necessarily tedious operation until the *seventeenth day*, when death resulted very suddenly from syncope.

It is, happily, a rare occurrence to meet with a calculus of such magnitude, and the fatal termination was not to be wondered at. The removal of very large stones has been alluded to by the best lithotomists as a hazardous, and often fatal operation, the danger increasing in proportion to the size of the stone.

In Case V., in which I declined performing any operation, the sufferer had been cut on two occasions by the lateral method; once in a county infirmary, and once in a Dublin hospital. The poor boy was evidently the subject of the "phosphatic diathesis," and had all the symptoms of advanced renal disease. I found difficulty in sounding his bladder, in consequence of the presence of urethral stricture, caused by the two cutting operations; the bulbous extremity of Thompson's sound could not be passed through the obstruction. A slender instrument was used for the purpose of measuring the stone, which was found to be about the size of a pigeon's egg; it was so soft and adhesive that it was most difficult to disengage it from the sound; this was at last effected by passing the left forefinger into the rectum.

Case VI.—When admitted into hospital the patient was suffering from chronic inflammation of the bladder of unusual severity, accompanied by prolonged hæmaturia. There was no evidence of kidney disease. Under judicious treatment, local and constitutional, the vesical catarrh was subdued, and the patient's health sufficiently restored to enable me to sound his bladder and measure the stone. The symptoms of inflammation and constitutional debility were evidently produced by *local irritation* of stone in the bladder of a phosphatic nature. The successful result of the operations in this case far exceeded my most sanguine expectations.

In concluding these remarks on the operations of lithotomy and lithotrity, it may not be unimportant to note the chief practical points for the surgeon's consideration in selecting the operation suitable for a case of stone.

In the child, lithotomy is, as a rule, to be preferred, in consequence of the narrow calibre of the urethra, the hard nature of the calculus (it being often of the mulberry variety), and the extreme sensibility of the genito-urinary organs at this period of life. For these reasons the crushing operation is, in most cases, unsuitable for children.

In the adult lithotrity is preferable, but only under the following favourable circumstances, and then it is one of the safest of the capital operations in surgery:—The stone being neither too large nor too hard, and one or not more than two calculi present in the bladder; the urethra being healthy and of full size, so as to freely admit the passage of instruments without causing rigors; the bladder being tolerant and free from disease; the prostate gland normal or not much enlarged; the absence of albuminuria or pus in the urine in large quantity.

Cases of stone (as No. V.) will occasionally be presented to the surgeon, in which it will be his painful duty to decline any kind of operative interference—advanced renal disease placing the patient beyond the resources of surgical skill.

ART. VI.—*Description and Illustration of an Ether Inhaler for the Inhalation of Ether as an Anæsthetic, with a few Observations upon a Mixture of Chloroform and Spirit of Wine for producing Anæsthesia.* By B. WILLS RICHARDSON, Fellow and Examiner, Royal College of Surgeons; and Surgeon to the Adelaide Hospital.

THE relative safety of chloroform and ether as anæsthetics having been recently brought before the profession in Dublin by my friend, Mr. John Morgan, at the first meeting of the Surgical Society of Ireland for the present Session, it appeared to me that it would be advisable to have constructed a simple and moderate-priced inhaler for etherization in hospital practice (Plate XII.). This inhaler has been designed as a substitute for the towel and sponge, in the use of which there is much waste of ether, a matter, in hospital economy, that may be of some importance.

Although I have not myself adopted ether to the exclusion of chloroform, having had most satisfactory anæsthetic results from the use of a mixture of chloroform and spirit of wine, still I think it is our bounden duty, the relative danger of chloroform over ether having been so prominently raised, to test the point, that we may aid in the attempt to solve this deeply important question.

It is almost needless to write, that a question requiring such accurate and unprejudiced observation for its solution as that of the relative anæsthetic safety of chloroform and ether cannot be settled in a day.

To satisfy the logical mind it would be necessary to administer the two agents to an equally large number of persons, and to the same class of persons, having either injuries or diseases similar in their nature. As this, however, would be impossible, we can only arrive at an approximative estimate of the relative safety of these anæsthetics.

Whether or not ether is destined to recover its long-lost prestige in the United Kingdom remains, therefore, to be proved. At all events, Mr. Morgan deserves credit for the talent and energy he has applied to the solution of this important, and if I might venture to call it such, biological question.

According to Snow, about a fluid ounce of ether is usually inhaled by an adult patient in becoming insensible; but in order that the administrator may not, if possible, have to refill the box, it is made of a size sufficient that an ounce and a half of ether when

in the box will fill it to near the upper margin of the diaphragm. It is necessary, according to Snow, that the inhalation should commence, as in the case of chloroform, with the expiratory valve of the face-piece of his inhaler turned to one side, that the vapour should be admitted to the air passages by degrees, to avoid the irritation that would arise from suddenly inspiring any considerable quantity of the vapour. In Plate XII. the admission of air is regulated by the sliding or rotating cap A. When the inhalation has been in progress for one to two minutes, the patient should be made to breathe nearly altogether the pure vapour, which is effected by rotation of the cap A. If there be no peculiar susceptibility against ether narcosis, the latter will be complete in a few minutes.

The quantity of ether my friend Mr. Morgan pours into his ingeniously-constructed inhaler is from one and a half to two ounces. For the description of this excellent apparatus, and for much interesting matter on ether as an anæsthetic, I must refer the reader to his very instructive monograph on the subject.*

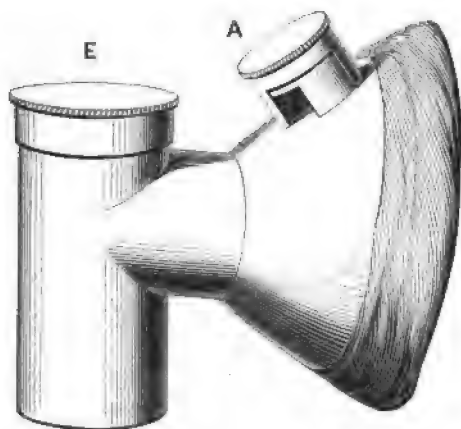
Although deaths, it must be conceded, have resulted from the inhalation of ether, I will, notwithstanding, reproduce here the following observations of Snow regarding the safety of ether, as an inducement for the trial of ether by those who are not disposed to do so in consequence of their having had good luck in the use of chloroform.

"I believe," he has recorded, "that ether is altogether incapable of causing the sudden death, by paralysis of the heart, which has caused the accidents which have happened during the administration of chloroform. I have not been able to kill an animal in that manner with ether, even when I have made it boil, and administered it quite pure." "I hold it, therefore, to be almost impossible that a death from this agent can occur in the hands of a medical man who is applying it with ordinary intelligence and attention."

When we compare these observations with the information collected by Mr. Morgan^b regarding the safe anæsthetic properties of ether, we feel constrained to observe that he would be prejudiced indeed who neglected to test the relative anæsthetic safety of chloroform and this agent.

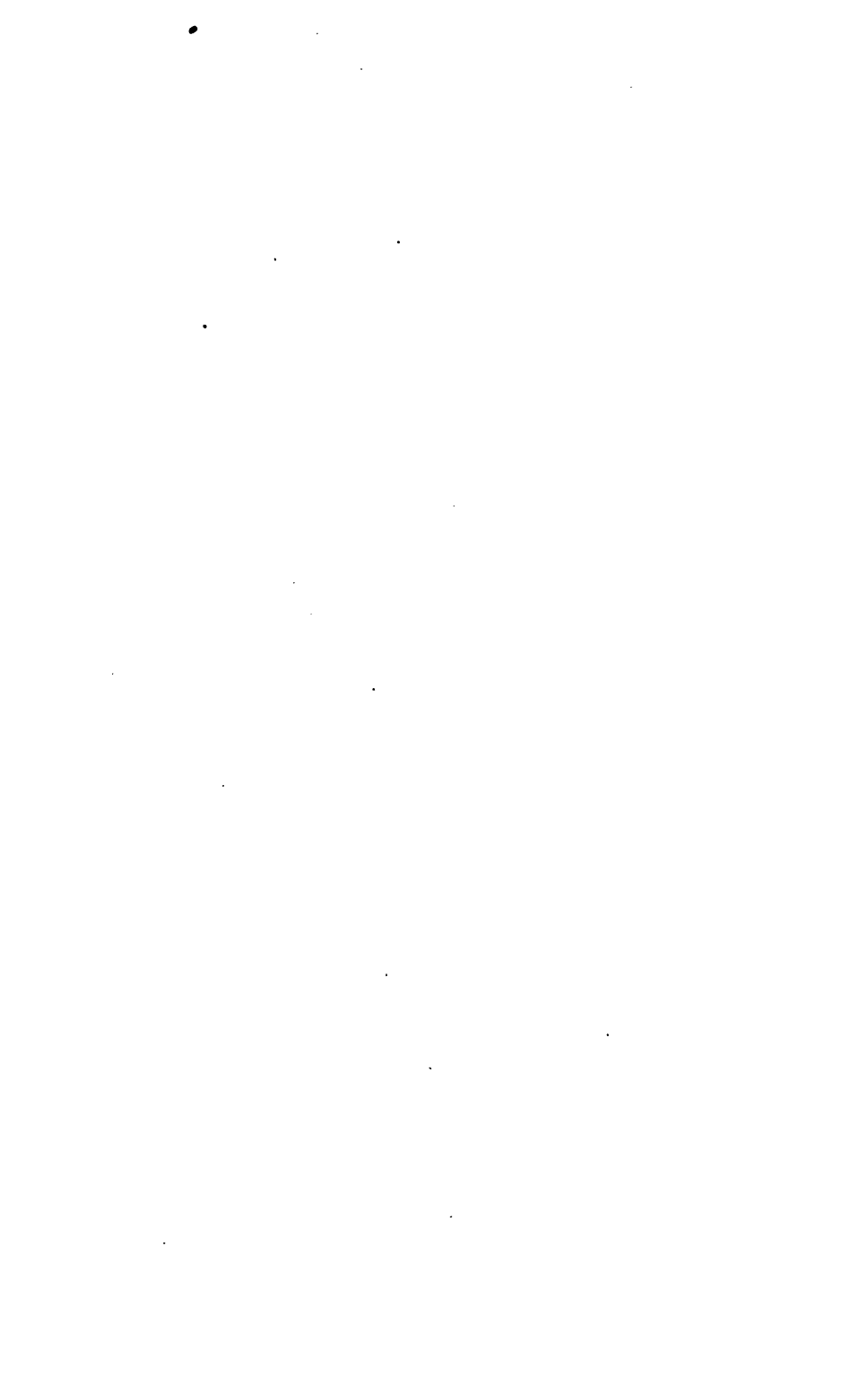
* *The Dangers of Chloroform, and the Safety and Efficiency of Ether.* By J. Morgan, M.D., F.R.C.S. London: Baillière, Tindall, and Cox. 1872.

^b *Ibid.*



A.—Air opening. The amount of air to be admitted to the face-piece is regulated by a sliding cap, having an opening of the same size as that in the tube on which it turns. At the beginning of the inhalation the inner opening may be fully exposed and gradually covered, by rotating the cap according as the air passages of the patient become accustomed to the vapour. E.—Ether-box, to hold a little more than an ounce and a half of ether. This box communicates with the face-piece by means of a tube an inch in length and one inch and a half in diameter, the ether-box opening of the tube being two-thirds closed by a fixed diaphragm. This prevents the fluid ether from passing into the tube when the patient is in the horizontal position. The face-piece opening of the tube has a diameter of one inch. The tube itself, in order to increase the evaporating surface, should be nearly filled with soft cotton candlewick, having, when in use, one end submerged in the fluid ether. The inhaler may be made of silvered copper or of block-tin; but the face-piece margin should be formed of flexible metal and covered with morocco leather.

MR. RICHARDSON'S Ether Inhaler.



I confess that I am myself in favour of a mixture of equal parts of chloroform and spirit of wine for producing anæsthesia, and, therefore, it is the comparative safety of this fluid I purpose testing against the ether. This mixture is best administered with either a piece of lint, a handkerchief, or with Skinner's apparatus, for the following reasons mentioned by Snow:—"Whenever chloroform is administered from a handkerchief or sponge, it should be diluted with one or two parts, by measure, of spirit of wine or eau-de-Cologne. In this way the convenience of the handkerchief or sponge can be taken advantage of without the danger. Very little of the spirit is inhaled, as the greater part remains behind after the chloroform has evaporated; but it acts by lowering the elastic force of the vapour of chloroform, and diminishing the amount of vapour which is given off from the handkerchief or sponge, just as diluting a strong solution of ammonia with additional water diminishes the amount of the volatile alkali, which escapes as gas. One hundred cubic inches of air are capable of taking up fourteen cubic inches of vapour, at 60°, from pure chloroform, and becoming expanded to 114 cubic inches; but when the chloroform is diluted with an equal volume of alcohol, it will only yield eight cubic inches of vapour to 100 of air. In passing over a sponge or handkerchief the air would take up less than this, usually four or five per cent., which is a quantity sufficient to cause insensibility in four or five minutes, without the risk of sudden accident."

I do not recollect that I have seen this mixture produce insensibility in so short a time as Snow mentions.

In conclusion, may I observe, that in the present medico-legal aspect of the relative safety of ether over chloroform for the production of anæsthesia, it would not, perhaps, be injudicious, upon the part of the administrator, to permit the patient to select the anæsthetic.

ART. VII.—*A New Mode of applying Pressure in Popliteal Aneurism.* By HENRY THOMPSON, M.D., Surgeon to the Tyrone Infirmary.

SOME years ago, having had occasion to treat a case of popliteal aneurism, and neither possessing, nor having any means of procuring within the necessary time, the clamps and other appliances hitherto employed for the purpose, I was obliged to devise some

plan, by which the object could be accomplished by the means that were at my disposal, and, after some little thought, I adopted an apparatus similar in principle to that of which a woodcut accompanies this brief notice. Precluded from the use of the screw, I did not see why another of the mechanical powers, the lever, might not be brought into requisition, particularly as by a simple expedient, well illustrated in the woodcut, it would be easy to adapt to it Carte's principle of elastic pressure.

The plan was found quite successful, as far as its power of controlling the circulation through the femoral artery was concerned, and would certainly have effected a cure, had the other circumstances of the case admitted of it; but the patient had a thoracic aneurism, accompanied with a diseased state of the blood, which prevented coagulation, and of which he ultimately died.

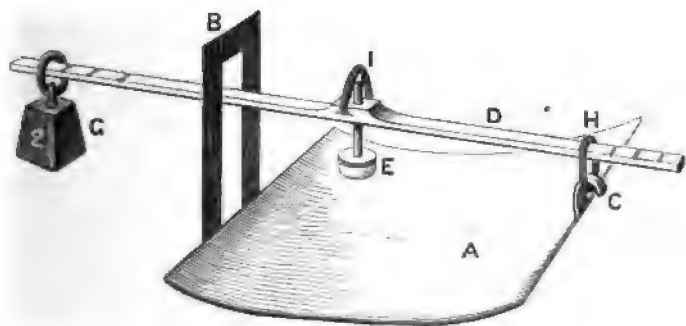
The progress of the external aneurism was, however, so materially checked by the use of the instrument as to prove its power of doing all that was requisite under more favourable conditions.

Within the past two months another case has occurred to me, which has given me an opportunity of verifying my anticipations, by the production of a complete cure of a popliteal aneurism by the use of this instrument alone. I have, therefore, no further hesitation in recommending it to the profession, as possessing some advantages over those hitherto in use: First, It adopts the principle of elastic pressure to the fullest possible extent.

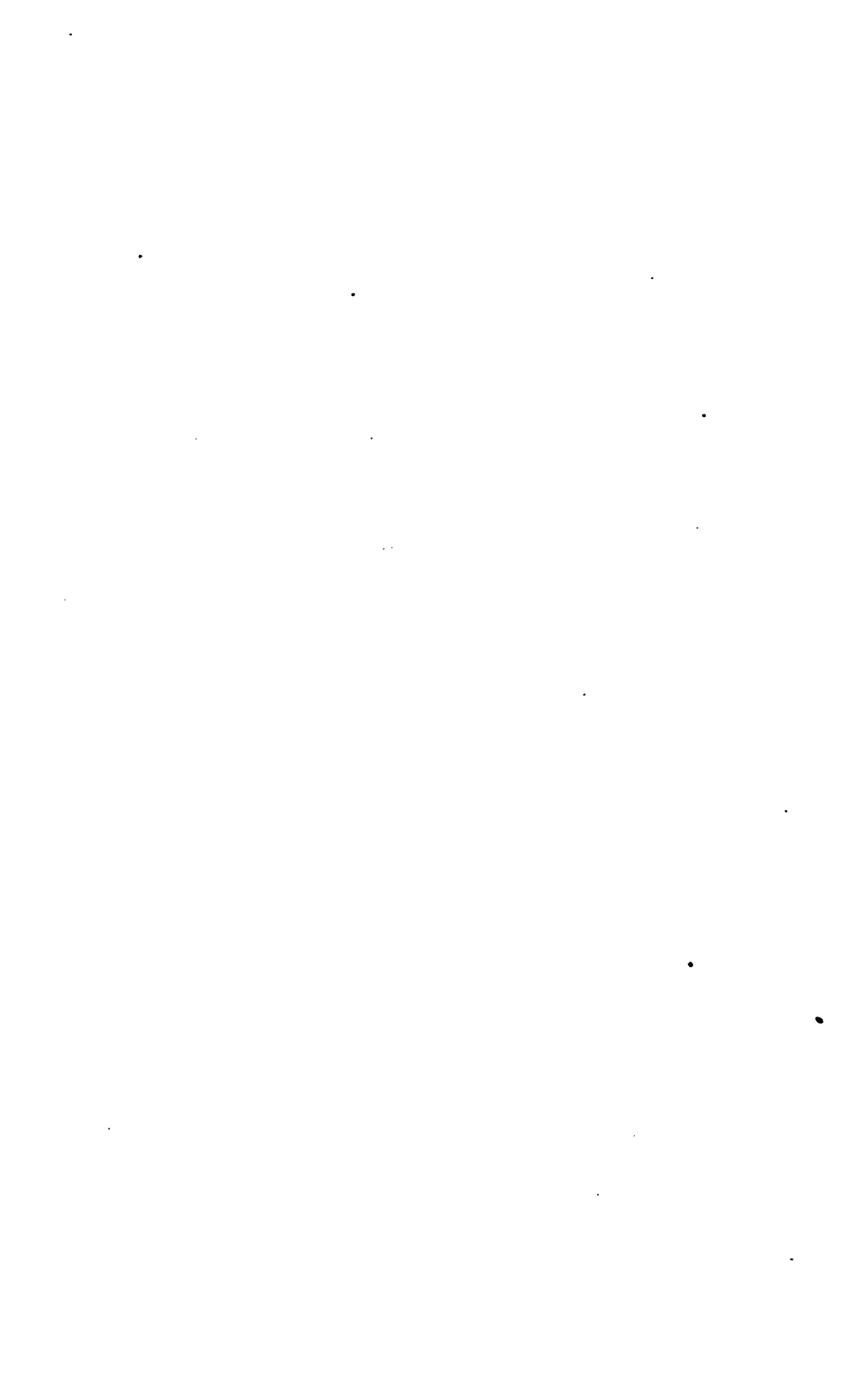
Second, It is easily made by any one who can cut sheet iron; and

Third, It can be watched and managed by the patient himself with the greatest ease and comfort.

As illustrated by the woodcut, the instrument consists of A, a piece of sheet-iron, 14 inches by 10, hollowed and padded to receive the pelvis, and furnished with B, a portion of the same piece of sheet iron turned up perpendicularly, and a slot cut in it to receive the lever. C, another piece of the sheet-iron exactly opposite the slot, turned up and bent down into a hook, to hold a strong elastic ring. H, to form the fulcrum. D, the lever, a thin lath of deal $2\frac{1}{2}$ feet long, $\frac{1}{4}$ inch thick, and $1\frac{1}{2}$ inch wide, with a square mortice cut in it at I, where its thickness should be doubled. This mortice is to receive the stem, which supports the compressing pad E, and the opposite end of the stem is fixed to the sides of the lath by a stout piece of elastic band which passes over the end of the stem and is tacked to the sides of the thick part of the lever. G is a two-pound



DR. THOMPSON'S Instrument for the Treatment of Popliteal Aneurism.



weight slung over the free end of the lever, forming the compressing force, which can be increased or diminished by moving the weight out or in. The position of the pad on the vessel can also be changed as required by moving the pelvis an inch or two up or down, which the patient soon learns to do for himself without relaxing the pressure on the artery. With this apparatus I have kept up continuous pressure for 12 hours without interruption.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Science and Art of Surgery: being a Treatise on Surgical Injuries, Diseases, and Operations. By JOHN ERIC ERICHSEN, Senior Surgeon to University College Hospital, and Holme Professor of Clinical Surgery in University College, London. Sixth Edition. London: 1872.

THE rapidity with which "Erichsen's Surgery," first published in 1853, has gone through five editions is, perhaps, as good a proof as any that we can offer of the practical utility of the work, as a text-book for students, and as a book of reference for the practitioner.

The sixth edition, now issued, is carefully revised throughout, in order to bring the account of each branch of surgery as far as possible into accordance with modern progress.

It is easy to discover in this work, or in any work which deals with a similarly extended range of subjects, actual faults and many expressions of opinion to which we might take exception, but, in our opinion, it contains more correct and practical information, with less error or useless information, than any of its present competitors.

In the chapters on inflammation and suppuration the sections relating to the pathology of the processes are re-written, and embody the results of the researches of Cohnheim and others on the action of the blood corpuscles, their passage through the walls of the vessels, and the origin of the corpuscles of pus from the white blood cells. Sufficient information on this subject is given to allow the reader to obtain a clear idea of the extent of the knowledge of these changes, without entering into details on the many controversies at present unsettled on the subject, which would be out of place in a text-book of practical surgery.

The chapter on the arrest of arterial hæmorrhage contains a short account of the chief modifications of the ligature which have been tried from time to time, from which we quote the following, as it expresses the author's ideas on the subject of the most recent and most ardently advocated modification:—

"The idea of the employment of ligatures made of animal substances that would admit of absorption, and thus allow the wound to be immediately closed over the noose, so as in fact not to act as foreign bodies in the wound or as agents of suppuration, still occasionally presented itself to the minds of surgeons; and, amongst others, Velpeau speaks of it with favour, admitting, however, that their precise nature and form have to be determined. Of late the use of catgut has been revived by Lister in connexion with his 'antiseptic method' of dressing wounds. He uses catgut soaked in carbolyzed oil, and has reported favourably of its employment. But more widely extended experiments are required to determine its real value. For it is impossible not to be struck by the remarkable fact that, in all the various attempts at the modification of the ligature and the substitution of unirritating or absorbable materials that have been made during the past fifty years, the good results that have so often, and in such varying circumstances, been obtained by the inventors of a new method, have failed to be secured by other surgeons, or even maintained by themselves."

It would appear, then, that Mr. Erichsen's experience, and indeed general surgical experience, proves the extravagance of the following statement of Lister:—

"That by applying a ligature of animal tissue antiseptically upon an artery, whether tightly or gently, we virtually surround it with a ring of living tissue, and strengthen the vessel where we obstruct it. The surgeon, therefore, may now tie an arterial trunk in its continuity close to a large branch, secure alike against secondary hæmorrhage and deep-seated suppuration."

In the chapter on special amputations we notice several changes both in the text and illustrations, and we think that it is to be regretted that more care has not been shown in this part. Many improvements have been made in the text, while many might still be made, especially if the author's intention is to make this part of the book a sufficient guide for the student, or for an inexperienced operator. Amongst the improvements we may notice the introduction into the text of Mr. Syme's own description of amputation through the ankle-joint, in place of the account given in previous editions by the author, which was most inaccurate.

Two imposing illustrations of amputation through the shoulder-joint are substituted for the small woodcuts in former editions, and it seems strange that the error of the first, which represents a knife transfixing the upper end of the humerus, with directions given to

the operator "to cut by a sweep downwards and outwards" through the bone, should have escaped a practical surgeon. Still the change of the woodcuts shows that the author considered some improvement on previous editions necessary, while he has succeeded only in making the fault more striking.

A strange error is continued in this edition too, in reference to amputation of the finger—a small matter, but one still of sufficient importance to require notice.

"Amputation is performed between the proximal and second phalanges in the same way (that is, by dorsal and palmar flap); but, as a general rule, it should not be done here; because, as no flexor tendon is attached to the proximal phalanx, it is apt to remain permanently extended, and a good deal in the patient's way. In case of the index finger, however, it will be better to leave the proximal phalanx, the stump of which forms an useful opponent to the thumb."

We should be inclined to reverse the rule here laid down except the last clause, and say, that this proximal phalanx should not be amputated, unless the patient desires it for mere appearance sake; certainly not with patients of the labouring class, for, in the majority of cases, it forms a useful and perfectly movable stump. The mistake in this case has arisen from an imperfect knowledge of anatomy, for the flexor tendon is attached sufficiently to the proximal phalanx at its distal end by the retinaculum to retain its action on the bone; and again, if this fail, or is cut (it should be saved if possible), still the tendons will be generally attached in the stump and their power will remain, or even the interossei may act to flex the joint when others fail. Any way, experience proves amply that the power is not lost unless in cases where disease has destroyed the tendons and joints. The operation we contend for is infinitely less troublesome to the patient than one which opens the palm of the hand, to some degree at least.

Again, we must notice an illustration representing the mode of compressing a femoral artery with the fingers, which appears for the first time in this edition; we should, perhaps, have said "digital compression," because otherwise we should be convicted of a bull, for the drawing represents the thumbs as the compressors. Now, we hold that the woodcut exhibits just the wrong one of two methods, and just the mistake which we have often seen cause an operator much embarrassment during an amputation. The thumbs are represented as making pressure in a direction parallel to the

sloping surface of the ramus of the pubis, instead of in a direction vertical to that surface, and this, we hold, makes all the difference between inefficient and efficient pressure.

While many inaccuracies such as these occur in the work, and, as we have noticed, even in the new matter introduced, its great merits are the completeness of it as a surgical manual, while the judgment of the author as a practical surgeon is well and clearly expressed in the various controversies of modern surgery.*

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1. *Skin Diseases : their Description, Pathology, Diagnosis, and Treatment.* By TILBURY FOX, M.D., Lond. Third Edition, re-written and enlarged. H. Renshaw, 356, Strand, London. 1873. Large 8vo. Pp. 532.
 2. *A Treatise on Diseases of the Skin, and its Appendages.* By Dr. AUSTIN MELDON. London: Longmans & Co. 1872. Pp. 270.

DR. TILBURY FOX's well-known work is hardly to be recognized in its altered form, for it has grown from the modest proportions of 32mo^a to those of large 8vo, and is now well brought out and excellently illustrated. Notwithstanding this increase in bulk we do not think that the popularity gained by the former edition, both in this country and abroad, will be risked by the change which has been made. Although many will doubtless regret the handy and compact little red volume, so convenient for reference, the improvements effected in the present edition are so considerable and so real, that, in this instance, his readers will readily excuse the author for departing from Mr. Syme's rule of making each successive edition of a book smaller than the preceding one. No less than sixty-seven new illustrations are introduced, which are clearly delineated, and two forms of disease are described in detail for the first time under the terms *fibroma fungoides* and *dysidrosis*. A chapter is added on the anatomy and pathological histology of the skin; the classification is rendered more symmetrical; the varieties and nomenclature of skin diseases, that curse of dermatology, are materially simplified; and, in fine, Dr. Fox has embodied in his work a full and practical account of cutaneous diseases, treated in his characteristic colloquial style, while the advisability of polishing his diction has evidently

* A few copies of the small edition are still to be had for the use of students.

not entirely escaped the author's attention. The text is so thoroughly re-cast that it would be impossible to strictly compare this edition with the last, and we shall accordingly but glance at the points in which the chief improvements have been made.

One of the most conspicuous features of the work is the careful attention devoted to treatment, and herein Dr. Fox's book stands out strongly in contrast with the otherwise admirable treatise of Neumann, who, for example, disposes of the treatment of that troublesome affection, urticaria, in a few lines. The directions for the treatment of psoriasis and eczema are particularly full and satisfactory; and the article on the latter disease is a condensation of the Lettsomian Lectures for 1869-70, which were noticed in this Journal for August, 1870. The pathological sections are much amplified, and the morbid histology of the skin is brought up to a recent level. Full use is made of the researches of German and American pathologists, many figures from Neumann and others are freely drawn upon; but, in the description of the vegetable parasites, the author furnishes original drawings. Dr. Fox has done good work in the clearing up of some disputed points, and we may especially instance his account of the forms of *lichen*, of *prurigo*, *hydroa*, and *lupus*; and in the latter disease, he rightly draws the distinction between the *lupus erythematoses* of the Germans and the disease described under that name by English writers. The characters and management of the syphilides are better given; the form of tertiary disease termed *dactylitis syphilitica* is described and figured after Dr. Taylor, of New York; Hebra's account of *rhinoscleroma*, a peculiar new formation about the nose, is inserted, and two figures are furnished of a very ugly form of disease allied to *fibroma moluscum*, and which the author designates as *fibroma fungoides*. On the subject of *senile changes in the skin*, Dr. Fox follows Neumann, and contents himself with giving a *résumé* of his observations; and, in the section on *phthiriasis corporis*, the elaborate investigations of Schjödte are quoted, which show that the pediculus possesses a species of sucking apparatus, and not a mouth with mandibles, as has generally been supposed. Lastly, the importance of a correct knowledge of ringworm is testified by the increased space allotted to its varieties, and the author holds stoutly to his belief in the existence of a parasitic form of *alopecia areata*, although most of the recent researches tend rather to classify it as an atrophy of the hair. From a careful review of the whole book we feel assured that Dr. Fox has produced a work of solid merit—one which we

have no hesitation in recommending as the best text-book we possess on skin diseases, and which is well adapted to the needs of the young student and the experienced practitioner.

In the handsome volume in which Dr. Meldon makes his *debut* as a dermatologist we have an outline of the principal forms of skin disease, apparently designed chiefly for the use of beginners. The author has such a horror of systems of classification that he solves the difficulty by adopting none at all, and sketches the various affections in an almost random order; but, for the benefit of the curious, a table of the leading classifications which have been proposed is appended. Although neither the matter nor the orthography of the work would stand severe criticism, Dr. Meldon has succeeded in presenting, in a simple and readable form, the elementary points in reference to the ordinary diseases of the skin.

Notes on the Treatment of Skin Diseases. By R. LIVEING, M.D., Cantab. Second Edition, with additions. London: Longmans, Green, and Co. 1871. Pp. 104.

ON its first appearance Dr. Liveing's handy and unpretentious little volume of "Notes" met with a cordial reception on all sides, and a second edition naturally followed in due course. In it little alteration has been made beyond the addition of a few articles, *e.g.*, Furunculus, Hyperidrosis, Vaccinia, Varicella, and Diseases of the Nails, and we hope that if future editions are called for the author will see the wisdom of not expanding the present size of his useful and convenient work, which we again recommend with pleasure to students and practitioners. It is not a mere medley of compilation, but is a really good condensation of what is most certainly known of the best treatment of skin diseases, while the therapeutical formulæ are judiciously selected, and furnish a reliable repertory for general use.

Disease Germs; their Nature and Origin. By LIONEL S. BEALE, M.B., F.R.S. Second edition. London, 1872. Pp. 472.

THIS work consists of three parts, which, in the former edition, were published separately. The first treats of the supposed nature

of disease germs, the second, of their real nature, and the third, of their destruction, and of the treatment of diseases caused by them.

The author, dismissing in a few words the view that contagious diseases are caused by a non-living ferment substance, devotes the first part of his work to an attack on the vegetable germ theory. Passing over this, we will go on to the second part, in which he states his own views.

There is a very peculiar substance found in the bodies of animals and vegetables, which, by most physiologists is called protoplasm, and which Dr. Beale used to call germinal matter, but which he now calls bioplasm. This is, according to him, the only living matter in the world, and from it are derived, not only all the tissues of plants and animals, but also all the formed elements of the morbid growths and exudations which occur in living beings, and among these the disease germs in question.

This bioplasm has the power of spontaneous movement, it grows and multiplies by division. Under certain circumstances, its particles, none of which ever attain a very large size, surround themselves with a hard coat, which is known to most anatomists as the "cell wall," the contained bioplasm being known as the cell contents, or the outer part of the mass may undergo change into some of those things which we call tissue, as bone, cartilage, muscle, &c., &c., while the imprisoned portion of bioplasm continues to grow from the centre, its outer part continually undergoing the tissue change; these little portions of bioplasm, which lie thus scattered through the tissues, are known to all histologists as the bone corpuscles, cartilage cells, muscle nuclei, and so on. Besides these fixed portions of bioplasm we find others free, and which are known as the white corpuscles of the blood and lymph. Now this bioplasm is endowed, according to Dr. Beale, not only with the powers already enumerated, viz., of spontaneous movement, growth, division, and transformation into highly complicated tissues, but it possesses also a peculiar property called life, by which it is able to act in opposition to all the known laws of nature. It can generate heat without the expenditure of any equivalent force, and, although specifically heavier than the air, can rise vertically through it, apparently much in the same way that, by psychic force, the furniture and bodies of the spiritualists are in the habit of doing.

Now, while we freely admit the properties of bioplasm which we first spoke of, the movement, the division, the share in the formation of tissue, we confess we think it premature to assume that all these

are inexplicable by ordinary laws, and that a new and mysterious force must be postulated for their explanation. It is true there are a great many things which we are still unable to explain by the laws of chemistry and physics, but the number of these is diminishing every year, thanks to the very spirit of disbelief in the vital force which is abroad. Notwithstanding Dr. Beale's contempt for the spirit of the age, and the tendency of modern thought, we do not believe that we should better ourselves by going back to the metaphysical stage of reasoning, and resting content by referring everything we could not understand to some imaginary force or virtue or essence. By the admission of such an entity, which is, *ex hypothesi*, a mystery, and transcends our powers of knowing, a stop is put to investigation, and a wide field is thrown open to theories and speculations of the most fanciful kind. We cannot but think the work before us is an instance of the danger we allude to. We find throughout theory carried far in advance of what the facts justify, and a most singular ignoring of the work which has been, and is being, done by physiologists in this and other countries. Dr. Beale thinks that observation of living protoplasm deserves more attention than it receives; but he must surely be aware that for many years past the living tissues and cells of the animal body have been the constant subject of observation by hosts of microscopists, and it has occurred to us that if Dr. Beale himself had examined the living tissues more, and confined himself less exclusively to observations on preparations stained with carmine, he would have hesitated to put forward some of the views propounded in this and his other works.

Dr. Beale finds, on examining the tissues of animals and men who have died of febrile diseases, that there is a great increase in the quantity of bioplasm which they contain beyond that found in health. This bioplasm exists not only in large masses, such as we are accustomed to call cells and nuclei, but also in very minute particles, some not more than the $\frac{1}{1000000}$ of an inch in diameter. Besides this increase of bioplasm in the tissues he finds a similar increase in the blood, and many of the smaller vessels appear closely packed with white corpuscles and smaller masses of bioplasm, and their cavities narrowed by the projection into them of the enlarged nuclei of their walls.

These are the facts; the rest of the work is chiefly theory.

In the healthy body the bioplasm of each part has special properties and can produce tissue only of a particular kind. Thus one kind of bioplasm can produce only muscle, another fat, another bone,

and so on; although all these kinds of bioplasm are indistinguishable by any known test, one from the other. But, under certain abnormal circumstances, any kind of bioplasm may take to growing and multiplying, and may produce a number of little masses which are incapable of undergoing transformation into tissue, or of any other kind of development. These are what we are in the habit of calling pus corpuscles, and the bioplasm composing them has, by its rapid growth and multiplication, lost many of its special powers. Now if the growth and division of the bioplasm proceed further, very small particles result, many of which are not more than $\frac{1}{100000}$ of an inch in diameter; these are not only, like the pus bioplas^{ts}, good for nothing, but they possess virulent properties; they are, in fact, disease germs, and their entrance into the body of a healthy person is followed by the phenomena of some of the zymotic diseases. From this it appears that the germs of disease are nothing foreign to the body, but that they are direct descendants of healthy bioplasm. It is not so clear to what they owe their virulent properties. If we were to conclude, from a diagram on page 256, we should say that it was from the rapid multiplication of masses of bioplasm from a common ancestor, and in the numerous plates we find objects labelled disease germs apparently because they stain red with carmine, but we suppose besides these characters there is, in the disease germs, a modification of the vital force, which, being a mystery, must be received without explanation. As already mentioned, one kind of bioplasm cannot be distinguished by any test from another, so that the substance of the most innocent leucocyte is indistinguishable from that of the most virulent small-pox germ. As every part of the body, both solid and fluid, contains particles of bioplasm, it is evident that the difficulty will be almost insuperable of proving that those in the bodies of persons who labour under contagious diseases are really the active agents in spreading the contagion. We find throughout the work before us a confusion between morbid conditions, which are, to our mind, very different one from the other, and what is observed in one of these conditions is immediately applied to explain the phenomena of the other. Thus, ordinary local inflammation, as from injury, or an anthrax, non-infectious febrile diseases, as common cold in the head, infectious fevers, as typhus, and cattle plague, and even such diseases as cancer, and tubercle, are all mixed up together, as if they were mere varieties one of the other. It may be true that in all of these an increased production of bioplasm

occurs to a greater or less extent, but this would seem to prove only that if this phenomena is common to all these conditions, so different in every other respect, it must be unessential, at least in some of them, to the essence of the disease.

Dr. Beale does not say that any of the contagious fevers, at present, originate in the patient himself by degeneration of his own bioplasm, but, from his theory, we suppose he must admit its possibility, although in most cases they are produced by inoculation with disease germs of some other sick person. These germs being very small and possessing the active habits of bioplasm, easily get into the body through any chink in the cutaneous or mucous surfaces. There is no use wearing respirators or in covering the body with impervious materials, for, do what we will, there will always be some unprotected spot large enough for a germ visible only with a $\frac{1}{30}$ th, to find an entrance through. But if the body be in a perfectly healthy state, the germs will not find in it a suitable soil, and the inoculation will come to nothing, hence the importance to all those who come much in contact with the sick, that they should keep themselves in the best health possible.

Shortly after the entrance into the body of the disease germs, a great multiplication of these takes place. Whether this is due to an increased growth of the pre-existing bioplasm of the body, or to a rapid growth of that of the germs themselves, the author does not decide, although he inclines to the latter view. However, of this he has no doubt, that the high temperature in these cases is not owing to oxidation or any common-place physical cause, but is due to the change of dead matter into living bioplasm, which wonderful transformation is always attended by the evolution of heat.

It is in the blood that the disease germs chiefly multiply, and the capillary vessels are plugged up and occluded partly by the circulating masses of bioplasm, partly by the enlarged masses in their walls. To this obstruction of the capillaries Dr. Beale attributes most of the severe and dangerous symptoms of fever. Now in this part of his work we think the author has gone hopelessly astray. There is no evidence whatsoever to show that such an obstruction occurs at all, but all the evidence we have points just the other way. If the supposed obstruction were to occur its first effect would be to increase the tension of the blood in the arteries, and to diminish that in the veins, to empty the venous system and to fill the arterial. But from the very commencement of the

fever the contrary condition prevails, the arteries are too empty and the veins too full; with the rise of temperature the arterial tension falls, and the more intense the fever the lower the tension sinks. This lowering of tension is produced in two ways, first, by the weakness of the heart, which sends the blood feebly, and in small quantities into the arteries, and this feebleness of the heart seems to be due directly to the high temperature of the body; secondly, and chiefly, by a relaxation, due probably to sympathetic paralysis, of the small capillary arteries. Through these dilated tubes the blood is allowed to flow so freely into the veins that the weak heart is not able to maintain the difference of tension in the arteries and veins which is essential for the due carrying on of the circulation. It is to this dilatation of the peripheral arteries that we would attribute the greater part of the disturbances of the circulation in fevers. It is true that there is deficient capillary circulation, but it is not because the capillaries are obstructed, but because the *vis a tergo*, derived from the tensely filled artery, is wanting to drive the blood through them. The analogy with the small vessels of inflamed parts is quite inadmissible. In inflammation the capillaries do get full of white blood corpuscles which accumulate there as the circulation gets slow, and whose accumulation is the consequence, and not the cause, of the slowing; but this slowing of the circulation is compensated for by a more rapid flow in the vessels around the seat of inflammation, and here the small vessels are relaxed and the arterial tension low, as shown by the pulsation which can be felt in the small arteries, and sometimes even in the veins. We repeat that we think Dr. Beale has adduced no evidence to prove that any accumulation of white corpuscles sufficient to obstruct the capillaries throughout the body ever occurs in fever.

The germs, after having increased and multiplied in the blood and tissues, leave the body. Dr. Beale will not allow that the glands take any part in eliminating the febrile poisons, although he admits that they may throw out certain imperfectly oxidized materials, which, if retained, might serve as pabulum for the disease germs. In this part of his work the author combats, at great length, the view that gland cells are destroyed in the act of secretion. Except in the case of the sebaceous and mammary glands, this view is not supported by much evidence. At the same time we think all evidence is against another theory which we find Dr. Beale holding, namely, that the solitary glands of the intestine and

the follicles of Payer's patches periodically burst and discharge their contents into the digestive tube.

As well as we can understand Dr. Beale, he supposes the disease germs to make their way, by their own powers of locomotion, to the surface of the body, and there to escape through some of the pores in the skin or mucous membranes, leaving, however, a few behind, which, and their descendants, continue to circulate in the blood, and, absorbing all the nutriment fit for the nourishment of similar germs, protect their host from a second attack of the same disease.

In the third part of the work before us, which treats of the destruction of disease germs, we have an account of all the disinfectants and of the modes of using them. Of these carbolic acid seems to find most favour with Dr. Beale. In the chapter on the treatment of fever there is not much calling for notice. Several pages are occupied in advising persons who suffer from a febrile cold to bathe their feet, take a hot drink, and go to bed. It is recommended to cool fever patients by covering them up with plenty of blankets, in order to determine blood to the surface and encourage sweating. Dr. Beale seems to forget that something more than a large supply of blood is necessary to make the sweat glands secrete, and that when the skin has a temperature of 104° there cannot be any lack of blood at the surface. In cases, however, where the temperature is excessively high, he advises the use of the cold bath, the employment of which in fevers has produced some of the grandest therapeutic results of recent times. In the sections on the use of alcohol there is not much new or calling for remark.

This book bears throughout the evidence of having been written more for the general public than for the profession. We think that it will not advance Dr. Beale's reputation among physiologists or those who are capable of forming a judgment on the subjects treated of. The fact of its having already reached a second edition is sufficient proof of its popularity. But in this, as in many other instances, we fear that popularity cannot be taken as an index of merit.

It is with much regret that we have written as we have done of this work. There is no one who knows better than we do, or is more willing than we are to acknowledge, the great value of many of Dr. Beale's contributions to medical science, but the authority which his great reputation gives to all his views makes it the more

incumbent on the critic to speak out when he believes these views to be erroneous. The interest of the subject treated of, the pleasant and easy style in which Dr. Beale always writes, and the extreme beauty with which this book is published, will, we doubt not, make it very popular with those whose acquaintance with biology is small. But, unless we are much mistaken, the feeling among scientific persons will be one of disappointment that Dr. Beale, from whom so much better might have been expected, should have produced such a work.

On the Theory and Practice of Midwifery. By FLEETWOOD CHURCHILL, M.D., Dubl. and Edin.; M.R.I.A.; Fellow and ex-President of the King and Queen's College of Physicians, &c., &c. Illustrated by 126 highly-finished wood engravings. Sixth edition, thoroughly corrected. London: Henry Renshaw. Dublin: Fannin and Co. 1872. Small 8vo, pp. 804.

IT is scarcely necessary to do more than announce the appearance of the sixth edition of this most excellent hand-book; but we cannot do so without adding that we have examined it carefully and find the author has spared no pains to make it fully represent the teaching of the present day. The book is thus made in every way worthy to hold the place hitherto accorded it—as one of the best manuals of midwifery. The fact that it has reached a sixth edition, notwithstanding the many rivals it has had to contend with, is the best proof that can be adduced of its value. There are not many medical authors who have had an opportunity of revising a sixth edition of their works so carefully as Dr. Churchill has done this one. We congratulate him on his success, and have no doubt each succeeding edition will make the book still more popular if as carefully revised as this one has been. We would suggest to him the formation of a completely new index in the next edition, as the present one is so defective as to make it difficult for a practitioner who, in an emergency, may wish to obtain aid from the valuable information contained in every paragraph of the book, to refer at once to the part he requires.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

HENRY EAMES, M.D., Honorary Secretary.

Wednesday, January 8th, 1873.

DR. GORDON, in the Chair.

On the Use of Digitalis in the Failing Heart and Delirium of Acute Diseases.
By JAMES LITTLE, M.D., Physician to the Adelaide Hospital, &c.

FAILURE in the force of the cardiac contractions is one of the most important phenomena in fever; it permits stagnation of the blood in the systemic and pulmonary capillaries, and so favours the occurrence of serious complications. Physicians in this city have for many years made it a special subject of clinical study. The publication of Dr. Stokes's researches on the condition of the heart in fever long ago drew attention to it, and Sir D. Corrigan's observations on the use of wine as the remedy for the lesions of circulation, cardiac and capillary, constitute one of the most valuable portions of his lectures on fever. The object of this communication is to bring under the notice of the Society the use of digitalis with the same object.

By the administration of nourishment in such quantities as the stomach can digest, and sometimes during critical periods, by the application of turpentine epithems, and other external stimuli, much can be done to support the failing heart, but our chief reliance, as all now admit, must be placed on alcoholic stimulants, and unfortunately they sometimes do harm if administered in quantity sufficient to effect the desired object.

1. There are persons whose brains are unusually susceptible to the influence of wine and brandy. I have observed this in those who had at some time sustained an injury of the head, several times in men who had been the subjects of sun-stroke, more than once in an individual who had

suffered from syphilitic head-ache, and once in a young man in whom there had been disease of the internal ear. In many cases it is not possible to assign any cause for this susceptibility beyond the vague one, that in some persons and in some families the head is the weak part, but its existence renders the management of the patient in whom it is present a very difficult matter when he becomes the subject of fever.

2. While previous indulgence in alcoholic stimulants commonly makes their use all the more necessary during acute illness, it is not invariably so. Habits of tipping sometimes render an individual very prone to exhibit the injurious effects of wine and brandy when he is ill.

3. Renal mischief, whether it arises during a febrile attack or has been of old standing, necessitates extreme caution in the use of stimulants.

If we attempt under any of these circumstances to use wine and brandy freely we find unfavourable symptoms are produced. Of these, some of the most common are flushing and heat of head, injection of the eye, head-ache, intolerance of light, restlessness, wakefulness, tremor, hyperæsthesia, and muscular rigidity. When the kidneys are affected we may have the still more alarming phenomena of suppression of urine, followed by convulsions and coma. According to my experience, digitalis not only helps to maintain the circulation, but has a tendency to relieve the cerebral symptoms I have enumerated, and to promote the secretion of urine, and hence it is specially useful in the very cases in which there are objections to the free use of wine. I have given it with the special object of increasing the force of the cardiac contractions in more than twenty cases of fever (of these, six were typhus, one rheumatic fever with typhoid symptoms, and the remainder cases of enteric fever), and although my experience of it is by no means large enough to justify me in speaking positively, it has produced a strong impression on my own mind, and leads me to solicit for the drug a trial at the hands of those whose observation of fever has been sufficiently extensive and sufficiently precise to qualify them for estimating the effect of the remedy. I give half a drachm of the tincture every fourth, third, or second hour; in some cases every hour. I have not seen any ill effects produced by it except sickness of stomach, and that only in the case I propose to narrate. I have never, however, continued its administration after the pulse fell to eighty, nor have I ever trusted to it, either in typhus or in enteric fever, to the complete exclusion of alcoholic stimulants.* I have invariably given along with the digitalis wine or brandy in small quantities, in quantities which may be described as cordial or stomachic doses,

* I would not be disposed to do so unless in such an exceptional case as the one I am about to detail, in which the alcohol had evidently a specially injurious effect on the brain, because I think it probable this agent is useful in fever in more ways than by stimulating the heart.

so that from two to six ounces of whiskey or brandy were consumed in the twenty-four hours. In one case of severe enteric fever and in two of typhus, it did not produce any effect, and was given up after the eighth dose; but in all the other cases, after six or eight doses had been given, the pulse was found much less compressible and its frequency diminished, in most of the cases by twenty beats in the minute; at the same time there was a diminution in the symptoms indicative of cerebral excitement.

In the case of rheumatic fever I trusted to the digitalis alone, and on this account, as well as because the case in other respects was one of interest, I have selected it as an illustration of the effect of the drug.

On the 22nd of October a merchant in this city, aged thirty-five, consulted me, suffering from subacute rheumatism. He had gone through three severe illnesses, the nature of which I could not ascertain, but in all of them sleeplessness and excitement had been prominent symptoms. He had taken wine and spirits freely, though not so as to intoxicate him. Six months previously he had been under my care for a group of symptoms, which had come on after exposure to a strong sun, and which indicated some cerebral mischief. They were occasional attacks of obstinate constipation with vomiting, constant feeling of heat and uneasiness in the vertex, and tingling in his fingers and toes. These symptoms had disappeared under a rigid dietary and the use of the iodide and bromide of potassium. He was also chronically dyspeptic, suffering at all times from thirst and flatulence. The rheumatic affection had begun as a creak in the neck a month before I saw him, and had been intensified by railway journeys, recklessness in diet, and general want of care. When I first saw him the pains were not severe, and I merely recommended a spare diet, with confinement to the house, and the use of an alkaline mixture. On the 26th he was rather worse, and I insisted on him remaining in bed. There was pain in nearly all his joints, but it was not severe, nor was there any swelling—the temperature being 102° Fahr. The urine and perspiration were not strongly acid; the pulse was weak, the tongue furred, and he suffered much from his habitual thirst and flatulence. I advised him to take twenty minims of the tincture of perchloride of iron every fourth hour, and Dover's powder at night. On the 30th there was a loud pericardial friction sound, and two small blisters were applied to the surface of the chest, outside the limits of the cardiac region. On the 1st November I found that he had not slept, owing to incessant muscular twitchings which came on when sleep was beginning. For this he had bromide of potassium with black drop. On November the 2nd there was no change, and I had the advantage of consultation with Dr. Head. We agreed to add to the tincture of iron chlorate of potass and quinine, with small doses of aconite. On November 5th he

had several liquid and very offensive stools. On the evening of November 6th the temperature was $103^{\circ}\cdot6$ Fahr., which was the highest temperature observed during the illness. On November 7th Dr. Head saw this gentleman again with me. We found that he had been consuming from eight to twelve ounces of brandy daily, but we did not think it right to lessen this as the sounds of the heart were feeble, and he was sweating and covered with sudamina. We directed a discontinuance of the iron, and gave him instead one grain of quinine, and one of opium, at intervals sufficiently short to relieve pain and secure some sleep. On the 8th and 9th these pills produced the desired effect, but on the night of the 10th sleep was not obtained until he had taken one every hour for seven hours. On the 11th he became violent, and from this day on the nightly excitement was intense. My notes made on the 12th, 13th, 14th, 15th, 16th, and 17th, show that bromide of potassium (thirty grains every second hour), a combination of Indian hemp, opium and camphor, and the hypodermic injection of morphia and atropia were severally tried without any satisfactory result in obtaining sleep. On November 18th he had been one hundred hours absolutely without sleep. He lay with his eyes open, talking incoherently, and often snoring as if he were asleep. When spoken to he was momentarily recalled to consciousness, and put out his tongue when desired. The face was neither flushed nor unnaturally pale; the pupils were dilated; subsultus and picking of the bed clothes were incessant, and in swallowing he often appeared to choke.

The first sound of the heart was absent, friction murmur had disappeared, and the pulse was 140 in the minute, and extremely feeble.

He took liquid nourishment freely, and passed several liquid offensive stools daily.

The urine had been scanty, but became fair in amount after the loins had been cupped; at no period of the illness was it albuminous.

For three days he had not had more than one ounce of brandy daily, and during this time there had not been any progressive failure in the pulse, but neither had there been any change for the better in the cerebral symptoms. Half a drachm of tincture of digitalis was now given every hour; after taking eight doses he fell asleep, and slept for two hours; during the night and following day the medicine was continued at intervals of two hours. On the 20th he was quieter, subsultus was much less; sometimes he was pretty rational, he had slept at intervals, and the pulse was 120, and steadier. The digitalis, however, had sickened him. During the next five days he had no medicine by the mouth, but every night I administered by hypodermic injection:—

Atropia, gr. $\frac{3}{10}$

Digitaline, gr. $\frac{1}{10}$

Morphia, gr. $\frac{1}{4}$

Under this treatment the first sound of the heart became audible, and the pulse steady at 110; he slept at night, and the muscular jerking ceased, and though slowly all the disturbances of the nervous system subsided.

The patient is now at his office, and without any physical sign of cardiac disease.

The PRESIDENT said this was a very interesting subject, and the point referred to by Dr. Little was a new one in the action of digitalis. He thought perhaps they had been too much in the habit of confining its use to organic diseases of the heart, and this new application of the drug was well worthy of attention.

DR. HAYDEN thought what was once said of the poet Campbell might be said of Dr. Little; it was a pity that they had not more of the valuable matter contained in his paper. No doubt the subject was one of great importance, and it was not asserting too much to state that digitalis was one of the most valuable therapeutic agents with which they were acquainted. The history of this drug it was not for him to undertake to lay before the Society, but he might say that from the time when M. Bouilland described digitalis as the opium "of the heart" up to the recent essay of Milner Fothergill and Dr. Foster, and the works of Brunton, the revolution of opinion as to the action of the drug was most striking, and he did not know that any more convincing illustration could be adduced of the progress of therapeutic science. He might say that at present digitalis was looked upon as one of those drugs the physiological action of which had been fixedly determined as stimulants and tonics of the heart. Dr. Little had given them some valuable information as to its action in acute disease, and the President had alluded to its action in chronic cardiac disease. Some years ago a doctrine that seemed novel was propounded in regard to the use of digitalis in delirium tremens by Dr. Jones of Jersey, who gave it in doses of two, three, and four drachms of the tincture, with alleged satisfactory results. He remembered what a revulsion was caused in the medical mind by this statement, but he believed that Dr. Jones stated what was perfectly true, and in such cases he (Dr. Hayden) had no fear of the alleged poisonous action of digitalis; given, however, in doses not exceeding thirty minims. The worst effect it produced was sickness of the stomach; but he had never given it doses larger than twenty or thirty drops, the average dose being ten, twelve, and combined with tincture of perchloride of iron and spirit chloroform. The former increased its action as a cardiac tonic, latter enhanced it as a diuretic. In delirium tremens one of the formidable symptoms was that of failing heart. A great many Dr. Ormond showed that this was associated with softening of

and they could easily understand therefore why digitalis would be beneficial in that disease. He would not follow Dr. Little in his observations as to acute disease, but, with the permission of the Society, he would mention one or two illustrations of the value of the drug in cardiac disease of another kind. In 1869 a man presented himself at the hospital with which he (Dr. Hayden) was connected. He was a very large, heavy man, and a careful examination of the chest satisfied his mind that he was suffering from a fatty heart, with dilatation of its chambers. There was scarcely any cardiac impulse. He had a tendency to sinking, and suffered greatly from paroxysmal dyspnoea of the most urgent kind, so that for a week at a time he could not lie down. Digitalis given to that man for three or four days so improved his condition that he was able to lie down, and the most distressing symptoms disappeared. He saw the man again and again at long intervals, and digitalis had always the same beneficial effect. In another case he remembered the pulse was 182, irregular and intermittent, and under the use of digitalis it came down to 96, and became quite regular, and the patient (a woman) was able to resume her ordinary avocations. He would encourage all who had to treat disease associated with weak heart, especially those where dilatation of the chambers existed, to take courage, and give digitalis freely. But at the same time its action on the stomach should be observed. It should not be given when the tongue was loaded, and the bowels in such cases should be acted on by mild aperient medicine. He should have mentioned that the digitalis was always beneficial only when it acted on the kidneys.

DR. GRIMSHAW said he once had a case similar to that described by Dr. Little, viz., a case of acute rheumatism in which digitalis was used. It was the first case in which he had employed that drug in acute disease. The patient was an exceedingly excitable, nervous person, but extremely susceptible to the action of alcohol. He was in the habit of taking a fair share of wine daily. The patient had three several attacks of rheumatic fever. He did not see him on the occasion of the first, and in the second attack he saw him when he was recovering. He had delirium but no cardiac affection. The delirium got worse as the rheumatism improved; the patient was constantly delirious, except when he was under the influence of considerable doses of digitalis; whenever digitalis, in considerable doses, was employed, he was comparatively quiet, but the moment any attempt was made to diminish the amount used, his delirium returned. He had an exceedingly weak heart, and after his recovery from the attack of rheumatism he still remained weak. Shortly afterwards, coming over from Kingstown to Holyhead, he received a severe wetting by incautiously exposing himself on the deck of the steamer, and this brought on another attack of rheumatic fever. He then had exactly the same

train of symptoms as on the former occasion. Digitalis was tried and it utterly failed to control the exceedingly quick pulse or increase the heart's power, and it was found the only thing he improved under was large doses of brandy, which did not aggravate his nervous symptoms but considerably improved them. The case assumed a form, which has been described by Dr. Ogle, of London, as pyemic rheumatism—purulent arthritis followed, and the patient died, becoming violently delirious before death. He wished to draw attention to the uncertainty of the drug. He had found the tincture more uncertain than the infusion, and he therefore preferred the latter. In delirium tremens digitalis would be found useful if not used excessively. He saw two patients treated with it without the precaution being adopted of using small doses at first, and both died after large doses were administered. He did not agree with Dr. Hayden, that the loading of the tongue should counter-indicate the use of digitalis. He believed from Dr. Little's statement there must have been loading of the tongue in the case, and yet he had used digitalis with a good effect. Perhaps there was a better drug than digitalis for weak heart; he referred to strychnine, which had been suggested to him by Dr. Henry Kennedy. Certainly, under strychnine, the first sound of the heart had been more effectually restored than under any other drug with which he was acquainted.

DR. HENRY KENNEDY confessed that after a considerable number of trials with digitalis, he had not been able to arrive at any distinct conclusion respecting it. He thought it important in recording cases treated by digitalis, that the particular form of the drug should be stated. In several papers recently published, a distinction was drawn between the effects of the drug in an infusion and as a tincture. He, himself, used the powder more than either of the other preparations, and it appeared to him to be a useful medicine. Digitalis was long in use for maniacal cases, and what occurred in Dr. Grimshaw's case of acute rheumatism, also occurred in these cases, viz., that the influence of the drug when being used, kept the patient sane, but when intermitted the maniacal symptoms came on again. He thought the case brought forward by Dr. Little scarcely bore on the question before the Society; for in that case digitalis had been used in combination with other powerful medicines. To accurately determine the specific effect of a drug it should be used by itself alone. He might mention that he had used digitalis externally over the kidneys, and with a very good effect. He thought great caution should be exercised in administering this drug, and that the advice of Dr. Jones, of Jersey, to give it in large doses, ought not to be followed. There were cases in which death followed so sharply on the administration of half an ounce of tincture of digitalis, that it must have been the cause of death. There were also cases on record where vomiting never ceased, and the

patient died from the result of vomiting caused by digitalis. The drug had been used in cases of phthisis, and held its ground for a long time as a mode of treating that disease. He was not disposed to place much reliance on experiments made as to the effect of drugs on the lower animals. It by no means followed that because a certain drug produced a particular effect on a frog, for example, it would produce the same effect on the human constitution.

DR. WALTER SMITH said that no class of therapeutic agents deserved more attentive consideration than the class of cardiac remedies—that is remedies which can act as excitors or depressors of the nervo-muscular tissue of the heart directly. Of this class, one of the number that had received most consideration was digitalis, and probably it was the one on which the most conflicting evidence had been published; but at the same time no such remedy showed more distinctly these two points—the inutility of therapeutic experiments on the lower animals, and the utility of them, combined with clinical experience in man. It was by the combination of these observations that the present position of digitalis had become recognized by the best observers as a direct cardiac stimulant. He was glad to find that Dr. Little, from his clinical experience, ranged himself in that category. Dr. Little had used very large doses of the drug, and that raised the important question—Was there a tolerance of digitalis in acute disease, as there was, admittedly, a tolerance of alcohol in certain cases? Can you give a patient, suffering under a certain acute disease, a dose of digitalis without an injurious effect, which in health would affect his heart, as in other cases you can give a dose of alcohol with beneficial effect, which in health would cause intoxication? The determination of that question turned on the preparation which was used. He thought one thing which tended greatly to the improvement of therapeutics, had been the recent adoption of acute principles or alkaloids, but there had been a difficulty in the case of digitalis. The name digilatine had been applied to different substances, and had been used in no more accurate sense than the word ergotine was used. Within a very recent period there had been isolated by a French experimenter what appeared to be the definite and active principle of digitalis—in other words, a guarantee of its purity. Dr. Little stated that he had injected one-fourth of a grain of digilatine. He (Dr. Smith) should like to know from what source this was obtained, or if there had been any guarantee of its purity.

DR. FITZPATRICK said the therapeutic value of drugs could only be accurately ascertained by clinical observation. Digitalis he believed was a medicine possessing various qualities—it was a sedative, a depressing agent, and a diuretic. Therefore, possessing these various qualities, it required to be studied with reference to the remedial view

which the physician took in administering it. For instance, he had over and over again given digitalis in a weak state of the heart, combined with ammonia, with most admirable effect, and yet he would never dream of giving digitalis alone in such a case, no more than he would of giving tartar emetic to a person dying. Given alone, and without the aid of stimulants, it might be productive of serious consequences, but, combined with stimulants and other medical agents to qualify its action, it was a most valuable remedy. If they depended on digitalis alone as a stimulant of the heart's action they would make a great mistake.

DR. M'SWINEY had had no experience in the administration of digitalis in acute disease, and consequently he would express no opinion on that point; but he wished to express his concurrence in the caution given by Dr. H. Kennedy, Dr. Grimshaw, and Dr. Fitzpatrick, that in the case of this drug, now justly esteemed so valuable in its therapeutic influence, the greatest care and watchfulness should be used in its administration. There was a danger that some young and enthusiastic practitioner might not watch its effects with sufficient care, and that unpleasant consequences to the patient might be the result. Five or six years ago he was called on to see a gentleman, the subject of apoplexy, in which case digitalis in full doses had been given by the previous attendant. He did not at first know what the previous treatment had been, but, being amazed at finding the pulse beating 35 per minute, he inquired into the matter, and found the patient had been taking half drachm doses of digitalis three times a day for eight days. He placed the gentleman at once on stimulants—ether, alcohol, and ammonia, enjoining strictly the recumbent position, and day by day the pulse rose until it came to 45 or 50. The gentleman declared he should get up, but Dr. M'Swiney warned him not to do so, and told him that if he rose he might faint, and if he fainted he might die. Five days afterwards he got up and walked or drove some miles to a friend's house, and when in the act of walking into the house he fell dead. He ascribed that death to a faint consequent on the feeble state of the heart, induced by the medicine. Therefore he strongly advised caution in the administration of this drug, which, he was bound to say, his experience showed was a most valuable remedy in suitable doses and cases.

DR. AQUILLA SMITH said Dr. Little's paper was of value as suggesting the importance of studying the therapeutic action of digitalis. No more striking example of the progress of therapeutics was to be found than the history of this very drug. They knew the general reputation it had for many years as being a sedative and a diuretic; but he had no doubt that, in investigating the action of the drug, it was essential, with the view of arriving at an accurate conclusion, that the practitioner should

be careful in selecting cases. The President alluded to the action of digitalis on the heart. No doubt it did act on that organ, but it required the nicest discrimination to select the cases of heart disease in which digitalis was curative. It had generally been found most successful in disease of the left side of the heart, particularly dilatation and hypertrophy of the left side. One of the indications for its use was tumultuous and irregular action of the heart. He was reluctant to apply the experiments on the lower animals to man, and to draw the inference that the result would be the same in both cases; but these experiments showed the action of the drug on the heart itself, and clinical experience in certain cases of heart disease confirmed them. All modern investigation went to prove that digitalis had no pretence to be regarded as a special diuretic. He thought there was a more powerful diuretic effect in a pint of Guinness's porter than in a large dose of digitalis. But it had no doubt an indirect diuretic effect in those cases where the heart was weak, for it improved the action of the heart and diuresis followed—the true explanation being that digitalis by its stimulative action on the heart removed the congestion of the vascular system, and in that way diuresis set in. In the same way opium might be considered to a certain extent a purgative, as there were certain cases where opium by relaxing spasms caused the bowels to act. No drug deserved more careful study than digitalis. All observers in England and France concurred in the opinion that it had a special action on the heart; but he agreed with Dr. Kennedy that, to ascertain its specific action with certainty, it should be given *per se*, in selected cases, uncombined with other remedies; and if, when thus administered, it was found to have a specific action, it might be combined with other adjuncts. When digitalis was given with wine, they could not really say to which of the remedies the cure was due. As to the preparations of digitalis, he believed all condemned the tincture. Another important point was that the drug should be given in small doses frequently repeated, say from half a drachm to a drachm of the infusion every three hours. Mr. Jones's heroic treatment was a mistake. Last year he (Dr. Smith) used it in cases where the heart's action was feeble, and found it most efficacious. Sir Dominic Corrigan published some years ago a paper on the treatment of epilepsy by digitalis, and lately he (Dr. Smith) had an opportunity of trying that treatment in an epileptic case. He gave three grains of the powder of digitalis in the form of a pill every night, and the patient had derived more benefit from it than from any other remedy. The interval between the attacks was now longer than it had been for some four or five years, and the gentleman had great confidence in the drug, and would not feel comfortable if he went to bed without taking his pill.

DR. PURSER thought that in acute diseases, where the circulation is feeble, this feebleness was due in many cases, not so much to weakness of the heart as to dilatation of the periplural arteries, which allowed the blood to flow too freely into the veins, and so tended to neutralize the real action of the heart, which is to maintain the difference of tension in the arteries and veins. He considered that many medicines, and digitalis among the number, acted by stimulating the sympathetic nerve, and contracting the small vessels about the body, and so making the cardiac contraction more effective.

DR. LITTLE, in reply, said he was much obliged to Dr. Purser for his observations, though they did not immediately affect the clinical question to be decided. He stated in his paper that he was anxious to bring this matter forward, because he had seen sufficient to make him ask those who had extensive experience in fever, and who had observed it with sufficient precision to be able to form an opinion as to the value of drugs, to give digitalis a trial. That description applied to no one more than to their Chairman, and he hoped therefore he would give it a trial. Some of the speakers had observed that it was impossible to judge of the value of a remedy unless it was given alone. In a case of typhus or enteric fever, with failing heart, he would not venture to discard wine and brandy, the value of which was known, and trust alone to a drug the effect of which was yet unknown; but he had seen such improvement in the steadiness and force of the pulse to follow on the administration of digitalis in some patients who had already been taking alcoholic stimulants, that he believed the remedy would in some cases be found useful. A few days ago he saw in consultation a lady in enteric fever, who was sleepless, and in whom the pulse was 120 and very weak, although she was getting six ounces of brandy and four of port wine. It was agreed not to increase the quantity of these stimulants, but to give her in addition half a drachm of the tincture of digitalis every fourth hour. When he saw her at the end of thirty-six hours she had slept, and her pulse was comparatively firm, and had fallen to 72. The digitalis was then omitted. In the case narrated in his paper, on the 18th, 19th, and 20th December, no medicine but digitalis was used. In that case he felt justified in trusting to the drug alone, because he believed that the alcoholic stimulants were unsuited to the peculiarly susceptible brain of the patient.

It had not been any part of his intention to allude to the use of digitalis in chronic diseases of the heart; experimental and clinical observations proved it to be a cardiac tonic. In animals poisoned by digitalis the heart was found rigidly contracted as if by a tetanic spasm, and not flaccid, as it would be if digitalis enfeebled the heart; and his own experience went to show that the cases in which its administration was hazardous were not

cases of weak hearts, but cases of powerful hypertrophied hearts. Two years ago a man who was under his observation died, he thought, from the use of digitalis. He had permanent patency of the aortic valves, with great hypertrophy of the heart, and having obtained relief of some of his distressing feelings from a mixture containing digitalis, he continued taking the medicine without advice, and was found dead one night. The feeble hearts were, on the other hand, the very ones in which digitalis was useful; he had seen patients with weak intermitting hearts take quinine and iron for three and four weeks without benefit, but improve in a few days when digitalis was also given.

The Society adjourned.

Wednesday, February 12th, 1873.

DR. DUNCAN, in the Chair.

Remarks on the Etiology of Enteric Fever, in Connexion with its Relation to the Strumous Diathesis. By HENRY KENNEDY, A.B., M.B., connected with Sir P. Dun's, and (lately) the Cork-street Hospital.

AT a recent meeting of the Pathological Society I exhibited a specimen of ulceration of the ilium, taken from the body of a man, aged twenty-seven, who had died in Sir Patrick Dun's Hospital, after having had, some weeks previously, typhoid fever. The case exemplified very well a point which I believe to be of much consequence to recognize, and which I would bring more prominently forward before this meeting, partly for the sake of discussion, which cannot take place at the Pathological Society; and chiefly because I had not time then to state, as fully as I wished, my views on the subject. The point I refer to is in relation with the etiology of typhoid fever; and amongst the many and important questions which arise in connexion with this affection, there are none assuredly of greater consequence, or, I may add, of greater difficulty.

In speaking of our fevers generally, and of their causes, it cannot fail to have struck any one who has considered the subject, that there is a vagueness about these causes which in itself is sufficiently embarrassing. Thus, over crowding and impure air are very generally set down as causes of typhus fever, as well as great atmospheric changes, and all the consequences which flow from want of food and clothing. It will be in the recollection of the meeting that some years since Sir Dominic Corrigan advanced the view that famine was the great and paramount cause of our many epidemic fevers—a view which he supported by apparently strong facts, but from which I ventured, with all respect, to differ, inasmuch as the facts bore a different interpretation, and one which, it seemed to me,

was much more in keeping with the records of the epidemic fevers which occurred elsewhere than in Ireland, to which country Sir Dominic confined his remarks. The truth is, the causes which by many are thought to be paramount in causing our fevers are too many, and the very utmost that can be allowed them is that they predispose to the engendering of fever, but that they will not actually excite it.

Now, in considering these predisposing causes of fever, it has always seemed to me that too much consideration has been given to one side of the question. Air, earth, water, sewerage, atmospheric changes, food and famine, vegetable and animal organisms, and other causes, have all been elaborately written about in connexion with the subject. The seeds of disease, if I may so call them, have been deeply investigated; but the soil on which they were to fall and take root has been scarcely recognized or taken into account at all; and yet I have the strongest conviction that this part of my subject is deserving our closest attention, and of throwing much light on the etiology of fevers, and on none more than the type of fever known as enteric or typhoid. It is quite true that the state of our frames, at a time when what is known as the epidemic influence prevails, has been long recognized as modifying sickness, and in a very marked degree. But I wish to push the idea much farther, and to show that the very fever itself undergoes a change, and this owing solely to the constitution or frame in which the fever is lighted up. This, then, is the point I have in view in the following remarks; and, without further preface, I shall now proceed to consider it. In doing so it will be necessary to begin by making a few general remarks.

In the first place, it may be observed, that the very same causes do not lead to the same results as regards the effects on our frames. I may illustrate this by supposing the cases of four persons, each of whom gets a wound of a similar kind. What are the chances that these will terminate in a similar way? One or two may heal up kindly; another may slough; another may get erysipelas; and still another tetanus. Or a man, after a slight sprain, exhibits gout; or after a fall, and without any wound, gets the terrible disease, diffuse inflammation. It cannot be doubted that in each and all these cases it is the state of the constitution which determines the result; and that had there been no tendency to gout or erysipelas these affections would not have arisen. But further still, a man breaks the neck of the thigh-bone, and dies at the end of ten or twelve days of a fever not to be distinguished from typhus; or he goes off in the same way after anthrax or carbuncle; or, indeed, after any surgical operation, no matter how trifling. No one here ever questions the fact that it is the constitution is at fault, and so leads to the fatal result.

Keeping these facts in view, let us see if there be any similar ones in our fevers; and as typhus is one with which we are all more or less

familiar, I shall, in the first instance, take it. Now, no one who has seen two or more members of the same family, coming from the same room and afflicted with this disease, can fail to have observed the marked differences, I might almost say contrasts, which they present. This is not the place to enter into these differences, whether it be the spots, the symptoms, the complications, or the duration of the attack. Suffice it to say they exist. Yet we cannot for a moment suppose that these differences are due to anything but a difference in the constitution of the patient; and this it is which impresses itself on the whole character of the fever. But these differences show themselves in what may be called another aspect. For typhus differs in different years—a fact which my predecessors in the Cork-street Hospital noticed again and again; and indeed the remark may be made of other diseases, which vary much in character at different periods, no matter what some of our Edinburgh friends may have stated to the contrary, and particularly as regards pneumonia, which one might suppose from their writings was a stereotyped disease, from which there could be no deviation. Is it necessary to remind you that the great Sydenham observed and taught differently? Anyone, too, familiar with the history of our recent epidemics in this country cannot fail to have observed the marked contrasts which they exhibited. Thus, the great epidemic fever of 1817–18 was very different in its characters from that of 1847–48, which latter some of us may recollect. But it is not merely different periods which exhibit different phases of the same disease. On the contrary, we have positive knowledge that typhus fever is, in London, a much more fatal disease than with us. My own impression, too, is the same as regards typhoid. At any rate, as regards typhus, the fact is known and admitted, and I believe it would hold true of other diseases; nor do I know that any more interesting paper could be written than one which would contrast the diseases of London and Dublin, for it seems to me there are some very marked differences between them. But I only make this as a passing remark; and coming nearer home, and in close connexion with my subject, I may remind you of the marked contrast in the rate of mortality which typhus presents, according as it attacks the poorer or the middle and higher classes. It is certainly more than double, if it be not three times, as fatal in the one as the other. Explain this as we may, it may be assumed that it is not due to anything special in the contagion itself, for such an idea would only be adding to the difficulties in the matter which already exist. To my mind, it is much more rational to attribute this difference of mortality to mental or bodily causes, or both combined, rather than a difference of poison, which only adds another difficulty where there is already enough.

The conclusion, then, I would draw from the several points—I may indeed call them facts—to which your attention has been directed this evening is, that the state of our frames, when poisons strike them, has a

most important influence on the symptoms, progress, and result of each particular disease; and that this view goes far in explaining many of the apparent anomalies which surround the subject.

It will now be seen, I trust, how what has been advanced can be brought to bear on the etiology of enteric fever. My hearers are aware that for many years past the cause of this type of fever has been considered as totally different from that which causes typhus. Many amongst ourselves hold it; in London it still keeps its ground; and my friend Dr. Murchison has even gone so far as to name enteric "*Pythogenic fever*," from the idea that dirt was the great leading cause of the affection. In this view I cannot, with all respect, concur. I believe dirt, in every form, to be a strong predisposing cause of fever, but that it will lead to typhus as well as typhoid. That the latter is the isolated affection, which many hold, I cannot for a moment suppose; and I rather think that some, if not a good many, now hold different views on this point from what they did some years since. I am not, however, about to enter here into the arguments which I think can be adduced in favour of the view that typhus and typhoid are the result of a common cause. This I have done on more than one occasion previously. But I cannot help alluding to one fact, which my friend Dr. Grimshaw has recently made out, viz., that typhus and typhoid will arise in the same room, and at the same time. Need I say how totally at variance this fact is with the statement of Sir W. Jenner. I may add, too, that every other point of specific difference between the two types of fever can, I believe, be as easily controverted as this one. I repeat, however, that the entering into such details would be quite out of place at present.

But if we assume for a moment that these two types of fever arise from one common cause, how, it will be asked, do I account for the different symptoms and progress of the two affections? How is it that they present such signs as enables us to distinguish them—the one from the other—in the great majority of instances? The answer to this question is the main object for which I come before you this evening; and keeping in mind what has been already advanced, it will be understood that I believe it is owing to the state of the constitution that the differences between the two types of fever are to be chiefly explained; and further, that enteric or typhoid fever is essentially due to the presence in the patient of more or less of the strumous diathesis. That constitutions vary in this respect admits of no doubt. Some exhibit the diathesis in a very marked degree, and others in a very trivial way; and so it is with the type of fever with which I believe it to be more or less closely connected. This idea may to most appear very far-fetched; but it seems to me to be sustained by a number of facts, which I shall now proceed to notice briefly.

There are, I imagine, few who have not been struck with the general

aspect of a patient labouring under enteric fever—I do not mean from the effects of the fever, but the general conformation of the individual. In the great majority of instances they will be observed to be persons of a fine skin, and many of them of a high complexion. Their frame and hands are delicately formed, and it is by no means uncommon to find marks of one kind or other on their persons. Thus you may see them bleer-eyed, or with spots on the cornea; or, if they be children, on the head, or, what cannot well be overlooked, and what has frequently come under my own notice, you will see scars on the neck, the result of enlarged glands, and suppuration following. In keeping, too, with these appearances, which, to say the least of them, betoken delicacy, we must keep in mind the duration of this fever, which seldom ends before the 25th, and is often prolonged to the 40th day. As regards relapses, too, it is singularly marked in its character, and in this respect is extremely like other affections which are avowedly strumous, such as strumous pneumonia or ophthalmia.

Of the symptoms to which enteric fever gives rise, there are two which call for notice here. The first is hæmorrhage, which is so frequent in this affection, whether from the nose, chest, or bowels; and yet this is just what we might anticipate from the nature of the constitution with which we have to deal. For it has always seemed to me that a frame tainted with struma is one, above all others, likely to exhibit hæmorrhage.

The second symptom of which I would speak is the affection of the lungs, which is such a frequent complication of enteric fever. This is not the place to enter into the details of this affection. Suffice it to say, it is one of a specific character; that it resists treatment in an infinitely greater degree than the analogous affection met in typhus; that it frequently puts on all the characters of phthisis, and in a certain percentage of cases does run into that disease. I am not sure that I could adduce any single fact which would bear more strongly on the point I am now discussing than the one just stated, and I know it has been met by others; and one gentleman in England, in the course of last year put on record different cases where phthisis followed directly on enteric fever. Long since I myself have done the same,* and I rather think Dr. Grimshaw can now give similar evidence. The number of cases too, where, short of the actual invasion of phthisis, the symptoms persist for an indefinite period, is very remarkable. On many occasions I have

* The case with which I began these remarks was one in point. The patient passed through enteric fever in June and July, 1872. In August he had not quite got rid of the fever, and within a fortnight began to exhibit symptoms of acute and very general bronchitis. Another fortnight was enough to show me that acute phthisis had set in. This rapidly carried the patient off, and on *post-mortem* examination both lungs were found crammed with miliary tubercles.

sent patients out of Hospital with the hope that the change would serve them. But this part of my subject may exhibit itself in another aspect, as the following case exemplifies:—

CASE.—A young man, of nineteen years of age, applied to me before Christmas, 1871. Different members of his family had died of phthisis, and there was no difficulty in making out that he also suffered from the disease. Under treatment he rapidly improved; that is, the symptoms all lulled, and he gained flesh, and became able to follow his occupation. In this way matters went on till October last, when I was asked to see him, and found him in bed, suffering from enteric fever in a well marked form. He had no less than three crops of eruption. After a very prolonged attack he got somewhat better, but I am sorry to have to add that the chest symptoms became then much more marked, and the poor fellow fell into what might be called a galloping consumption, which, in January of this year, carried him off. In another case, too, of enteric fever, which I saw with Surgeon Rogers, of William-street, where there was severe hæmorrhage from the bowels, I learned that two years before the patient had been threatened with phthisis, and had spit blood.

In opposition to all this again I may advert to the frequency of the bronchitis which complicates typhus; and yet I have not met a single instance where this was succeeded by phthisis. I do not say it does not occur, but only that I have not met it. On the contrary, I have seen two instances where I knew that phthisis existed. Both of these passed through a very severe attack of spotted typhus, yet in both the phthisis was left just as it had been before the fever.

In the last place I may advert to the great similarity which exists between the morbid states of the ilium and mesenteric glands, met with in phthisical patients, and the morbid state of parts, the result of enteric fever. To myself there seems no difference. If this be so, it will not seem strange that I consider them as the result of some common cause—at least until some more plausible explanation of the great similarity of the two morbid states be given. This idea in itself alone would lead to the conclusion that enteric fever was, or might be, called strumous fever. But I do not pretend to assert that the point is as yet settled.

The view brought before the meeting this evening has not been taken up hurriedly. On the contrary, it is the result of many years' observation; and in papers published long since I have adverted again and again to the same idea. Whether it be true in its entirety, as has been first said, I will not take on me to assert. But it may, I think, be assumed as proved that the presence of the strumous constitution, no matter in how slight a degree, is capable of materially modifying the symptoms and process of the type of fever known as enteric, and that it should ever be looked for. If the view advanced be correct it is quite plain to my mind that our prognosis will receive a degree of exactness

which it has never yet done, and that our treatment will be essentially modified by our knowledge of the nature of the constitution with which we have to deal.

Since these remarks were written I observe, in the last number of *St. Thomas's Hospital Reports*, London, a paper, by Dr. Harley, entitled "Tubercular Fever, and its Relation to Enteric Fever." I have not seen the volume yet.

The CHAIRMAN said the Society was much obliged to Dr. Kennedy for bringing this interesting and to him novel view of fever forward. In his own limited experience he could corroborate Dr. Kennedy's view, so far as to say that the occurrence of phthisis after typhus, was exceedingly rare. He had not seen it often after typhoid fever, but he could not say there was the same immunity after typhoid as there was after typhus.

DR. DARBY, had seen such an amount of fever in his life that he could not allow Dr. Kennedy's paper to pass without some remark. The question appeared to him to be, what was fever properly so called? He believed it to be a disease that ran its course, and terminated independently of all treatment, and the progress of which could not be stopped for half a minute. They could guide a person through it, but could not stop it. The great point for them to consider was, whether it was an idiopathic fever or a symptomatic fever, and he believed they had not yet come to a clear understanding upon that point. With regard to hæmorrhage, he believed that was to be accounted for by what he termed the medical tendency for the year. When there was purpura, there was a more than usual amount of hæmorrhages in fever cases. If it was a fever properly so called, all the local symptoms would disappear on the disappearance of the fever; and in such a case they did not treat by specifics, but by keeping up the general powers. He believed that in the treatment of all fevers, every year had its own peculiar features, which required modification of treatment. In one year one particular symptom was predominant, which might require some particular treatment. and he maintained that they could not lay down a hard line as to fevers.

The CHAIRMAN reminded Dr. Darby that the subject for debate was the etiology of enteric fever.

DR. DARBY did not believe there was a distinction between enteric and typhus fever. He did not agree with Dr. Kennedy as to the influence of strumous constitution. He believed, that whatever was the type of the prevailing epidemic, the fever would be of that type, whether the constitution of the patient was strumous or not.

DR. EAMES said Dr. Kennedy believed the predisposing causes, acting over a number of persons, excited cases of fever in some, and that it depended upon the constitution of the individual what fever he would

get. If of a strumous constitution, the poison would develop itself as enteric fever, and he (Dr. Eames) was delighted to find that Dr. Kennedy now acknowledged a difference between enteric and typhus fever, which formerly he did not. If a person of a strumous diathesis got enteric fever, he was more likely to get hæmorrhage than another, because the walls of the vessels were more delicate and fragile, and, therefore, more likely to rupture. He was not sure exactly what Dr. Kennedy meant when he spoke of phthisis, which was a generic term, including a number of diseases. One of the cases he had given was not of the tuberculous class of phthisis, but seemed to be a more chronic form of the disease. As to what Dr. Kennedy had said about the treatment of the disease, he (Dr. Eames) would only say that he believed it was generally held, that the duty of the physician in dealing with fever consisted as it were in steering the ship through the rocks.

DR. GRIMSHAW had seen a considerable amount of fever. He had been eight years attached to a fever hospital, and during that time had seen a great deal of typhus, of enteric, and of some other modifications of fever. To describe any disease merely as fever in the present state of medical science was not very precise. He did not think the term a satisfactory one. They must adopt a term that would include typhus and enteric fever, or drop the term altogether, and distinguish them as different diseases—namely, typhus and enteric fever. That there was a difference between them he was perfectly satisfied. There were a great number of predisposing causes and exciting causes that might give rise to fevers of different kinds—causes which, in fact, predisposed the patient to fever of every type, to all forms of zymotic disease, and nearly to every form of disease to which human beings are subject. He did not think, however, because typhus and enteric fever arose in the same family, in the same house, and at the same time, they necessarily arose from the same cause. He recently published a case in the *Dublin Hospital Gazette* of fevers of different kinds having arisen in the same family and in the same house. There was typhus and enteric fever, besides which there were two other cases that had all the symptoms of both diseases. He carefully examined that house, and found that all the conditions that had been usually considered as exciting causes of typhus fever existed, as well as those conditions usually considered to be exciting causes of enteric fever. The houses were overcrowded, and the inhabitants lived over a cesspool. Both of the conditions existed, and the result was that both fevers arose in that house, but there was no reason why the fevers should be considered identical on that account. As these people all belonged to the same family, and moreover were very like one another in personal appearance and complexion, it might be fairly assumed, if there was such a thing as tubercular diathesis, that

they all had it. Why, then, did some of them get typhus, and some enteric, and some both varieties of fever? Small-pox had prevailed in that house, and scarlatina had appeared there two months before. Were these all varieties of the same disease? He saw no reason for saying so. He had ascertained that those localities which were the favourite haunts of fever not only produced fever of various kinds, but produced cholera, small-pox, scarlatina, and, he believed in some cases, measles; but markedly small-pox and cholera were most prevalent in those places, and they never were free from diarrhoea from one end of the year to the other. Dr. Kennedy had referred to a paper of his (Dr. Grimshaw's), showing that thirteen houses had produced three kinds of fever. There was simple fever, a fever the symptoms of which were not sufficiently well marked to enable him to class it either with typhus or enteric; there were also typhus and enteric fever. He had ascertained that nearly all these thirteen houses had had cholera in them, and all suffered from small-pox. Take the case of Gill's-square, with seven houses, and they would find there was not an epidemic of any kind or form that had not appeared in that place. Cholera was there in 1832, and made a clean sweep of the inhabitants. Fever was always there, and scarcely a week passed without a fever case coming out of it to Cork-street Hospital. This proved nothing except that the conditions under which the inhabitants of that place lived predisposed to all forms of zymotic disease. When they wanted to look for the producing cause of enteric fever they must look to some locality where it prevailed without typhus, and see what the conditions were. This had been investigated in so many instances that it almost seemed to be a settled question. Perhaps the most remarkable instance of an epidemic of enteric fever arising suddenly, and proving extremely fatal, occurred in the village of Turling, in Essex. The disease spread through the village with extraordinary rapidity; it was exceedingly fatal, and no other case of fever occurred there. It was not stated that the inhabitants were all of the same diathesis, although many of them were nearly related, and indeed the first case of enteric fever that occurred was not a Turling person at all. There was one cause common to all of them—dirty water; it appeared that they all drank water polluted with sewage from their own houses. A similar thing occurred in Guilford, and it afforded a remarkable illustration in point. There were two water supplies in that town, one a high and the other a low level. The high level was supplied from the lower part of the town by a pumping arrangement from the bottom of the valley towards which the sewage flowed; the pumping well was polluted with drainage, and this water was pumped up to the upper part of the town, and the people in the upper part got typhoid fever, while the people in the lower town were entirely free from it until some time afterwards, when some people from the upper town, carrying probably

the disease with them, came to live there, when a few cases occurred. The water supply of the upper part of the town was interrupted for a short time by the breaking down of the pumping machinery, and during that time there was no typhoid fever until the supply of dirty water was resumed, and then typhoid fever occurred again. These facts clearly pointed to the conditions that favour the spread of fever. It could not be assumed that the people who lived in the upper part of Guilford had all the strumous diathesis, and that the people in the lower part had not a strumous diathesis. He held in his hand a book in which were tabulated more than 134 cases of epidemics breaking out in various towns. In every case where typhoid fever occurred the arrangements were such that the people drank water polluted with sewage from their houses, and where typhus occurred it arose from overcrowding. In the early part of last year he was in attendance on a large educational establishment. There were three buildings, all separate and detached, situated in different blocks. One of them was the room where all the pupils attended to receive instruction; in another of the buildings the female pupils resided, and in the third a large portion of the male pupils resided, but not all of them. In that building in which the class room was situated enteric fever broke out among the male pupils. None of the female pupils were attacked, although many of them had a strumous diathesis. The males were attacked totally independent of whether they lived in the building in which the class room was situated or in a detached building. When he came to investigate the question it occurred to him at once to inquire what condition was common to the males that was not common to the females? The males used water-closets, which the females did not use; the males drank from a water supply that the females never used; and the males got enteric fever, while the females escaped it. If the strumous diathesis theory meant anything, it meant that a certain number of those persons should have got enteric fever, and certain others should be free from it; but, unfortunately for the theory, the persons who got it were of every class and presented every variety of skin and complexion. He believed the large majority of the people in this country had the strumous diathesis, and if Dr. Kennedy's views were correct the liability to enteric fever should be very much greater than it was. He did not think, therefore, they could say that it was the constitution which determined the nature of the fever. On the contrary, the cases of Turling, Guilford, and many other instances justified the conclusion that typhoid fever arose from a special cause, distinct from that which gave rise to typhus. With respect to the alleged liability to phthisis after typhoid fever, proving that the person was of a strumous diathesis, he did not think it proved anything. If they adopted the views that had been lately advocated as to the production of some of the forms of phthisis, they would find on comparing typhus cases with

those of typhoid fever, that conditions favourable to the production of phthisis occurred in typhoid fever which did not exist in typhus. Thus they had in typhoid fever ulcerated surfaces and enlarged glands in the mesentery, with cheesy deposits; whereas in typhus these conditions did not exist. He believed that the fact of there being ulcerated surfaces, with enlarged glands and cheesy deposits, was much more likely to cause phthisis than any defect in the chest. He believed there was a great deal more of enteric fever in existence than they had any notion of. In the Registrar-General's returns diarrhoea was set down as the cause of death in a large number of cases, and he thought it highly probable that in many of these cases the real cause was enteric fever. He did not think they could fairly investigate the cause of any zymotic disease in a large town, for the conditions that favoured the spread of one zymotic affection favoured the other; and if they saw any locality specially marked as a fever locality, they would find prevalent there all the forms of zymotic disease to which humanity was liable.

DR. WILLIAM MOORE said he was a firm believer in the distinction between enteric and typhus fever. He could not say what they were, but that they were distinct diseases he had no doubt. If they took a thousand cases of typhus and the same number of enteric fever, the line would be found to be drawn as distinctly between them as it was between small-pox and scarlatina. He would mention the case of a public institution from which fourteen cases of typhoid fever came into the hospital with which he was connected about two years ago. They were all young men, averaging twenty-five years of age. The eruption had all the characteristics of typhoid fever, and there was not a single case of typhus among them. He believed fourteen more of the young men got the disease, and the establishment was broken up. An inquiry was made into the cause of the disease, and it was found there was a connexion between the water and the sewage they were using. This fever did not attack the strumous individuals, but took them *en masse*. He could not gather from Dr. Kennedy whether his cases had the characteristic eruption of enteric fever; but he agreed with him that he would be a very clever fellow, who, if two examples of ulceration of the ileum were placed before him, would be able to distinguish between them and say—"There is the ulceration of a long-standing fever, and here is the ulceration of tuberculosis." He had been seventeen years a colleague of Dr. Kennedy, and that was the first time he had heard him draw a line between typhus and typhoid fever. He had heard him over and over again say he could recognize no distinction between them. He (Dr. Moore) thought there was an enormous advantage in obtaining a correct knowledge of these fevers. Every one familiar with typhus knew that it ran a definite course, and that there was a relation between the intensity of the eruption and the intensity of

the disease. One most important point was the difference in the number of spots, there being a great number in some cases, while in others there were but few, and those that had the fewer eruptions were always the more severe. He had seen a patient with three or four hundred spots go through the fever quietly and well, while another who had but three or four spots went through a very severe attack with hæmorrhage. Dr. Grimshaw said the sequelæ of typhoid fever might be tuberculosis in some form, and he thought that that was so, for a long-standing fever like typhoid favoured the conditions which tended to tuberculosis, particularly in an abdominal form. He might mention that in the months of October, November, and December, he did not see a single case of typhus fever in Dun's Hospital. During that time there were not ten cases of enteric fever in the house, while on an average of three months the number was generally about forty. He attributed that remarkable absence of fever during the months he had mentioned to the great flushing of the sewers and cleansing of the streets by the heavy and continuous rain that prevailed.

DR. HUGHES said, however they might differ from Dr. Kennedy's views, all must admit that his communication was of great value, and his opinions ably supported; but putting aside the causes of fever which would open a wide field for discussion, he thought they would find the pith and marrow of Dr. Kennedy's communication in this proposition—that the patients' constitution when exposed to the virus of fever determined the form which the fever would assume. He had had some experience of fever, and during the present season, since the commencement of the clinical session, he had several cases of enteric fever under his care, and the peculiarity of them was, that every single case was complicated with bronchitis. That was a remarkable fact as contradistinguished from his experience of last year, when he attended a number of cases without any bronchitis. He could show Dr. Kennedy several cases now under his care, in which he could not point out anything that would indicate a strumous diathesis, no enlarged glands, ophthalmic appearances, or any of the other signs indicative of a scrofulous constitution, and yet all of them were affected with bronchitis. He could not support the theory put forward by Dr. Kennedy, that the individual constitution determined the nature of the fever. In November last he had a patient under his care, labouring under unmistakable signs of typhus fever, having well marked what was the best sign of it, the peculiar rash. In the same ward, and near him was his brother, and he was labouring under well marked typhoid fever. They came from the same house. Both of them left the hospital convalescent, and the patient who had had typhus, was now under his care again with the well marked eruption of typhoid fever, with the peculiar tongue, flushing of the face, and diarrhœa, which were characteristic of that disease.

DR. DARBY wished to know from Dr. Grimshaw whether the water supply to the public institution referred to by him was a constant supply for a number of years, and whether the fever that broke out there was a temporary occurrence? If the fever occurred at a particular time, and subsided within a particular period, and the water remained the same, it would weaken the force of Dr. Grimshaw's argument.

DR. GRIMSHAW replied that it was the fourth epidemic of the same kind arising in the same place under similar circumstances. The water was not always the same—sometimes being the Vartry, sometimes pump water.

DR. LALOR said the Society ought to be obliged to Dr. Kennedy for his paper, and to Dr. Grimshaw for the number of facts he had stated bearing on the important question before them. He (Dr. L.) was glad to learn that the dogmatism that existed some time ago as to the specific difference between typhus and typhoid fever was not now as intense as it formerly was. He had now been satisfied that a specific difference of the contagious cause in typhus and enteric fever had been proved. While admitting that Dr. Grimshaw had discussed the subject ably, he did not think he had dealt logically with Dr. Kennedy's arguments in reference to the specific difference in the cause of typhus and typhoid fever. He mentioned the case of a public institution which contained both males and females, and in which typhoid fever broke out on the male side, while no fever appeared at the female side. Dr. Grimshaw's argument was that the reason typhoid fever broke out and was confined to the male side of the house was that the sewage and water were bad. That would not prove that the specific cause of typhus and typhoid fever were distinct, for there was no fever at one side of the house at all, and there was typhoid at the other. He (Dr. Lalor) would beg to put forward a line of argument which was not usual. Assuming, for the sake of argument, that the specific cause of typhus and typhoid was the same, why was it that both varieties of fever occurred? He would suggest that in typhoid fever the invading poisonous matter, whatever it might be, was applied along the whole track of the mucous membrane of the intestines and produced its local effect. Suppose they could apply the same test to small-pox. What effect would the poison of small-pox produce if applied from one end of the mucous membrane to the other? Would any one say that there might not be the same difference in the variety of the disease caused in that way from the ordinary small-pox, as there was between typhus and typhoid? If they allowed the poison to go along the whole tract of the mucous membrane, would they not have extensive variolous ulceration of the intestines, and the eruption in the skin be scanty in proportion? The same analogy existed in all the exanthemata. If the eruption in measles and scarlatina was scanty on the skin, and if the fever be high, the eruption would be very profuse on the interior surfaces.

The CHAIRMAN thought Dr. Kennedy's paper was particularly good in this respect, that it expressed his views so distinctly that it enabled them to grasp the idea which he wished to bring before them. The arguments he advanced, so far as they were arguments, were very suitable to sustain his position; but he (the Chairman) confessed that the subject came upon him with surprise. It was some time since he had had an opportunity of seeing much of fever, but his recollection of his former experience was that he never could trace the strumous diathesis as being more marked in one class of persons than in another. He had for a long time considered that the two forms of fever under discussion were essentially distinct. One fact struck him as being worth a great many arguments, and he would mention one that occurred under his observation some time ago, and which bore upon the question. He had in hospital two cases side by side—one a well-marked characteristic specimen of typhus fever, and the other a well-marked characteristic specimen of typhoid fever. What was the subsequent history of those cases? They both recovered. One remained in the hospital; the patient who had had typhus took the typhoid fever, and the other patient, who had left the hospital, came back and reversed the case, taking typhus fever. He could not understand if constitutional differences had anything to do with it, why one of these patients had the strumous fever, and after a time the non-strumous form of fever. In his mind that circumstance placed the essential difference between the two forms beyond doubt.

DR. MORRISON said he had been a long time connected with a fever hospital in Newry, and he found that at the Spring season of every year, when people began to stir up their manure pits and to spread them over the ground, there was a considerable increase of enteric fever, and of that alone. It went through the families. He did not observe whether the individuals attacked showed any evidences of scrofula, or the reverse, and he attributed the great access of enteric fever at the particular period of the year to the stirring up of the manure heaps and the spreading of the manure over the fields.

DR. KENNEDY, in the course of a brief reply, denied that his views had altered as to the duality of typhus and typhoid fever. He always admitted that there were two types of fever that could be easily distinguished from each other, but he held that they might both arise from a common cause. He mentioned that in 1847, when relapsing fever was so prevalent, he had seen patients who had apparently recovered from an attack of fever, unaccompanied by spots, attacked again, even before they had left their beds, and on the second occasion the fever was attended with typhus spots; yet no one would venture to say that these fevers were due to different causes.

The Society adjourned.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIFTH ANNUAL SESSION.

JOHN R. KIRKPATRICK, M.B., Honorary Secretary.

DR. ATTHILL, Vice-President, in the chair.

Saturday, 11th January, 1873.

The Secretary read the following paper:—

Abstract of 280 Cases of Labour attended by WILLIAM D. HEMPHILL, M.D., F.R.C.S., Ireland, from the year 1840 until the year 1872.

—	No. of Cases	Mothers Lost	Children Lost
Labours natural, and without anything abnormal in mothers or children,	224	—	—
Breech presentations,	8	—	3
Footling,	1	—	—
Arm,	2	—	—
Hand and Head,	3	—	—
Twins,	1	—	—
Tedious Labours—second stage above 24 hours,	6	—	1
Operations,	11 ^a	—	—
Acephalous Fœtus,	1	—	1
Hydrocephalus,	1	—	1
Convulsions,	3	—	—
Puerperal Fever and Inflammation,	4	1	—
Puerperal Mania,	3	—	—
Hæmorrhage, Accidental,	3	—	2
Hæmorrhage, Post Partum,	6	1	—
Hæmorrhage from Adhering Placenta,	5	—	—
Adhering Placenta, without Hæmorrhage,	4	—	—
Lacerated Perinæum,	3	—	—
Total,	280	2	8

Remarks.—Under the head of natural labours, I do not confine myself to those cases strictly so called, but have excluded every case where anything abnormal has occurred to mother or child, during the progress of the labour ~~and~~ during convalescence.

re recorded; 9 of these were the removal of adhering Placenta, in another column also, accounts for incorrectness of total.

Breech Presentations.—Of these I had eight, or one in thirty-five, and lost three children. Two of the children were unusually large, first cases, and great difficulty was experienced in extracting the head. Of the children saved, one was a first case, one a seven months' child, one saved with difficulty—Marshall Hall's method being used for nearly twenty minutes before respiration commenced.

Only one footling—child saved.

Two arm presentations, one turned with difficulty, the arm having protruded for several hours before I saw the patient: one, a seven months' child, was expelled spontaneously while I was preparing to turn.

Head and hand, three cases, all children saved.

Only one case of twins, at seven months, the smallest living children I ever saw. Both died in a few weeks.

Had six cases of labour, the second stage lasting more than twenty-four hours. Mothers and children all saved. Two cases were remarkable, the mothers having had large families before, the children not unusually large, pains strong, and occurring at regular intervals, mucous membrane moist, in fact nothing to account for the great prolongation of labour. Four were first cases, with large heads, one of which required the forceps, the child being dead.

This was the only forceps case I had in private practice; if I had used it with some of the others, the second stage might have been somewhat shortened, but I did not regret allowing nature to complete the labours, as all the mothers recovered perfectly in the usual time, and the children were saved.

My only operations were the forceps case above mentioned, the version case, and nine removals of morbidly adhering placenta—mothers and children lived in all.

Met with one acephalous foetus, and one of chronic hydrocephalous—both children born dead.

Convulsions.—I attended three cases of convulsions, one in the year 1851, treated with bleeding and calomel, the other subsequently, with the vapour of chloroform applied with a handkerchief. I have every reason to be satisfied with the latter treatment—the convulsions in every case being stopped by the vapour without producing anæsthesia. The application was repeated every time the slightest symptoms returned. Mothers saved.

Puerperal Fever, &c.—I am happy to say we are fortunate in having very little puerperal fever in private practice in this locality. In one case the attack did not commence for three weeks after the confinement, although there was some malaise for a few days previously. Patient died on the seventh day from the attack. Treated with leeches, calomel, opium, and poultices, but could not produce pytalism.

There was an offensive water closet in a neighbouring house; patie

lived in a close part of the town. Three cases of puerperal hysteritis occurred on the third or fourth days after delivery—the operation for removing adhering placenta having been performed in each case. Patients were salivated and did well.

Had three cases of puerperal mania, one associated with *post partum* hæmorrhage and convulsions. All recovered, disease lasting from two to four months.

Hæmorrhages.—I have met with a good deal of hæmorrhages. Three cases of accidental. One caused by an accident, one by fright, and one without assignable cause. Mothers all recovered. Two children dead born, one saved.

Have had no case of unavoidable hæmorrhage.

Five of the cases of adhering placenta attended with more or less hæmorrhage. Mothers all saved.

Have had six cases of *post partum* hæmorrhage; mothers saved in five; one died in 1851. This woman carried a crippled husband on her back to the top of a very high house; labour soon came on (a few days before the time), attended with very slight accidental hæmorrhage; placenta came away in twenty minutes; put on a binder, everything being apparently all right; left the room but not the house, and was called up by the nurse-tender in about an hour and a half; found that she had lost a large amount of blood and was faint; on removing the binder an enormous coagulum came away. I gave ergot and used every exertion with pressure, and although I succeeded in checking the hæmorrhage, she never rallied, but sank in about an hour.

I had several patients who always expected flooding after delivery. On the recommendation of the late Dr. Montgomery I gave doses of sulphuric acid three times daily for nearly a month before the time, and I think with good effect. I have used the different solutions of ergot in those cases, commencing with a dose when the head is born, but although I generally use Long's glycerine solution in slight cases, I never in severe cases rely on anything but the fresh powder in infusion. This I have frequently given in half drachm doses, repeated even three or four times. But my invariable habit is never to relax my grasp of the uterus, commencing when the head is born and continuing pressure until the placenta comes away, and the binder with folded napkin is arranged.

I had three cases of lacerated perinæum, none requiring suture. On my attendance in subsequent confinements I found all as if laceration had never occurred.

I attended a large number of abortions, some attended with very great loss of blood, in which cases my invariable practice has been to plug the vagina. I never lost a patient, although some were brought very low. I find recorded in my case-book, as early as 1844, a case of abortion in which I plugged with a silk handkerchief. My late father, who had an extensive

midwifery practice for nearly fifty years, taught me the value of plugging, but I am indebted to Dr. Churchill, of Stephen's-green, for the very great improvement of plugging the vagina through the speculum.

I do not suppose there is anything either novel or unusual in the record of these cases, and their only value consists in their being a true statistical account of purely private practice amongst the upper classes, all cases occurring in public institutions, and amongst the poorer classes, where a physician is merely called in to give assistance to a midwife in difficult cases, but without having charge during their progress, being omitted. I have not included in the above list any cases where I have been called in consultation to the patients of other medical men.

Saturday, 8th February, 1873.

DR. SIBTHORPE, V.P., in the chair.

Intra-uterine Fibroid.—DR. KIDD laid before the meeting a specimen of a fibrous tumour removed the day before, and said this tumour presents some features of interest, which may, perhaps, render it worthy of being brought before the Society. In a paper communicated to the Society during the last session, I detailed a number of cases in which I had removed fibrous tumours from the interior of the uterus. Among the rest the case of a woman from whose uterus I removed, about two years ago, the specimen preserved in the bottle I hold in my hand, and which weighed six and a-half ounces. She had been much reduced by hæmorrhage before coming into the hospital, and at the time of the operation the uterus was very large, extending nearly half way to the umbilicus. After the removal of the tumour on that occasion, the hæmorrhage ceased, and the woman left the hospital and remained in good health for some months. The hæmorrhage, however, gradually returned; her menstrual periods became excessive, and she again sought admission a few days ago. The uterus was now again half way up to the umbilicus; the uterine sound passed into it five inches, and I determined to introduce sea-tangle tents and explore its cavity. On Wednesday last I made the first attempt to introduce the sea-tangle, but the os was so small, notwithstanding that I had removed this great tumour before, that I could only get in a single piece. On Thursday I introduced six pieces of sea-tangle, of No. 6 size, and on Friday I removed these and discovered the existence of a large tumour in the interior of the uterus. On examination I found this tumour was attached to the posterior and right side of the uterus, close to the fundus. It was attached at the same point as the tumour I had previously removed. Here is a sketch I showed last session of the former tumour, or, to speak more correctly, a diagram, as it was not drawn from nature, but only from the measurements ascertained on examination of the

uterus. That tumour was found growing from the posterior part of the uterus near the fundus, and at the right side. It was peculiarly lobulated with a broad base. I remarked to the Society the prominence of the uterus anteriorly, and mentioned that my experience so far led me to say that where a fibrous tumour grows in the uterus, and the uterus is prominent at one point, it will be found that the tumour grows from the side opposite. The specimen I show to-night is a marked example of this. The tumour grew from the same situation as the former one; the superior part of the posterior wall, and the lower part of the anterior wall was so bulged out as to resemble a greatly anteflexed uterus. The fundus seemed to be down under the symphysis pubis. This was a feature that to me was a very interesting one in this case. It is not merely a matter of curiosity, and I again wish to direct the attention of the Society to it, for if it be established as a law, it will facilitate the removal of these tumours, for it will enable us to say where the pedicle of the tumour is situated. This grew from the posterior and upper part of the uterus, and caused such a bulging out of the anterior wall, that when I examined it I could not get my finger round it, nor could I carry the écraseur round the tumour up to the pedicle. In operating on the first tumour, I succeeded in getting the wire round it, and encircled it at its base, and was thus enabled to remove it in one mass; but in the specimen I now show I was unable to do this, and had to remove it slice by slice. Five times I had to apply the écraseur before I got it all away. As the case corroborates the observation previously made with regard to the seat of the pedicle, I thought it right to bring it before the Society. Another feature of interest is that it is an example of the recurrence of fibroid tumour in the uterus. That is not a common event. Once we remove a fibroid tumour it rarely returns, but here in less than two years it has returned, and attained nearly the same size. The weight of the first tumour was six and a-half ounces; that of the present specimen six and a-quarter ounces. They are both well-marked examples of intra-uterine fibroid tumours.

On Chronic Inflammation of the Cervix Uteri: its Consequences and Treatment. By THOMAS MORE MADDEN, M.D., M.R.I.A., M.R.C.S., Eng.; Examiner in Midwifery and the Diseases of Women and Children in the Queen's University in Ireland; Physician to St. Joseph's Hospital and Dispensary for Sick Children; ex-Assistant Physician to the Rotunda Lying-in Hospital; Corresponding Fellow of the Obstetrical Society of Edinburgh; Corresponding Member of the Gynæcological Society of Boston, U.S.A., &c., &c.

CHRONIC inflammation of the neck of the womb is the most common of all the diseases peculiar to women. Thus, of six thousand three hundred

cases that came under my observation at the dispensary for diseases of women attached to the Lying-in Hospital, rather more than one-tenth belonged to this category, and in private practice I have found the proportion of these cases fully as large as in hospital or dispensary experience. The frequency of chronic uterine inflammation, producing ulceration and leading to hypertrophy, is only equalled by the importance of its consequences, and these are too generally disregarded by writers on the subject. Amongst the results of this complaint, not to speak of malignant disease, may be mentioned sterility, menorrhagia, dysmenorrhœa, vaginitis, leucorrhœa, hysteria in all its forms, and the long train of moral as well as physical infirmities which are the sequence of these morbid conditions.

The treatment of the affections now under consideration is still vague and unsatisfactory, generally extending over long periods of time, and often unrewarded by the cure of the disease, its predisposing causes being, as I believe, overlooked in practice.

I shall therefore now very briefly submit my views as to the causes of the frequency of these maladies, their effects, and the treatment they require, and venture to hope that the experience of even one individual who has had opportunities of observation may aid others who are entering on the same field of practice, and thus contribute something, however little it may be, towards putting the treatment of these affections on a more satisfactory footing.

II.—*Increasing Prevalence of Uterine Disease.*

No one conversant with medical literature in which is reflected the prevailing idea of the day as to disease, for there are fashions in professional opinion just as certainly as there are modes in dress—

usus

Quem penes arbitrium est, et jus et norma—

can fail, on comparing the medico-chirurgical publications of the present time with those of any former period, to observe the great prominence now given to the study of uterine affections, and the importance attached to them. The questions not unnaturally arise, have these diseases really become more common than was formerly the case? or is it merely the fashion of the day to ascribe all obscure complaints to uterine disorder, as it was formerly to attribute them to "the vapours," or "the spleen," or the more modern "nervous and bilious complaints," each of which was at one time so prolific a supposed cause of patients' ailments, and, more certainly, of physicians' fees? Or, have uterine complaints been always as prevalent, though only now discovered by the improved means of diagnosis furnished by modern gynæcological science?

Opposite as these doctrines appear, and warmly as they have been

espoused by conflicting authorities, there is, nevertheless, as in most other disputed subjects, some truth as well as some error in each of them. Uterine diseases are, I believe, more commonly met with as well as more in vogue, and, above all, they are more easily recognized than was formerly the case.

A comparatively short time has yet elapsed since either the frequency or the pathological importance of the morbid conditions now under consideration was first pointed out. The English writers of most repute on female complaints during the first thirty years of the present century, down to the time of Sir Charles Clarke, make no mention of non-malignant ulceration or congestion of the lower segment of the womb; and even as recently as 1857, Dr. Rigby, a well known and distinguished practitioner, asserted that "ulceration of the os and cervix uteri, not connected with malignant disease of the uterus, is in fact a rare affection."

The discrepancy of opinion between recent gynecologists and those who immediately preceded them on the question of the prevalence of inflammation, congestion, hypertrophy, and ulceration of the cervix and os uteri, may, to a great extent, be accounted for by the fact that the actual condition of the diseased part, which is now obvious to any tyro armed with the vaginal speculum, was, before the general employment of Recramier's invention, obscure, and only to be ascertained with difficulty and imperfectly by the light of general symptoms, assisted by tactile examination.

III.—*On the Use and Abuse of the Speculum.*

By the aid of the speculum, the uterine sound, the endoscope, and, above all, by the trained index finger of the experienced gynecologist, it is now as easy to investigate the protean maladies of the internal female organs of generation as it is to diagnose disease of the external parts of the body. Yet, comparatively few years have elapsed since M. Recramier's revival of the speculum or *διωπτρα*, the use of which was fully described by Paulus Æginata in the seventh century, and again given to the world in 1816, has been extensively employed. Even twenty years after that date, when Dr. Balbernie advocated the speculum, its use was almost unknown; nor did it find much favour with British practitioners till after the publication in 1844 of the first edition of Dr. J. H. Bennett's valuable work on "Inflammation of the Uterus."*

The vaginal speculum is unquestionably the greatest improvement that modern science has contributed to this department of medical practice.

* A Practical Treatise on Inflammation of the Uterus. By J. H. Bennett, M.D. 1st edition, London, 1845; 4th edition, 1861.

It is as essential in the treatment of many uterine complaints as the stethoscope is in the diagnosis of pulmonary or cardiac diseases. But it may be abused as well as used, and for my part I cannot approve of the indiscriminate resort to the speculum now so general in all cases of suspected uterine disease, and more especially when occurring in young unmarried women. I have had daily occasion for some years to induce as many speculums in hospital and dispensary practice as most practitioners, but yet I never regarded myself as justified in so doing in any case in which this instrument could be possibly dispensed with—that is, in which its use was most absolutely necessary for the diagnosis or treatment of the case.

Many cases have from time to time come under my observation in which the patient, having previously been treated for ulceration of the os uteri, insisted on being “examined,” and when, as not unfrequently happened, I did not think this procedure necessary, these persons were dissatisfied, and considered themselves neglected. And yet in a large proportion of such cases, by simply paying attention to the general health of the patient, ordering the free topical use of cold water, and enjoining total abstinence from marital relations, all the symptoms would subside, and the invalid would regain perfect health—the *mens sana in corpore sano*—far more completely and rapidly than if I had subjected the parts to the mechanical irritation of repeated examinations with the speculum, and so aided in keeping up that morbid concentration of the imagination on the supposed seat of the disease which is so common in such cases. But wherever extensive disease of the os or cervix uteri exists the use of the speculum is indispensable. Since M. Recramier's time a great variety of vaginal speculums have been devised. That most generally used in America, and of late by many practitioners in this country also, is Dr. Marion Sims' so-called “duck-bill speculum.” This instrument, although an admirable one for a variety of purposes, and more especially for all plastic operations in the vicinity of the vaginal canal, is, in my opinion, unsuitable for use in a case of simple ulceration or hypertrophy of the cervix uteri.

The objections to the “duck-bill speculum” in such cases are, first, that it is troublesome to use, necessitating the constant co-operation and aid of a trained assistant; secondly, that it causes more pain and greater exposure of the patient's person than the ordinary cylindrical speculum; thirdly, that the view it affords of the seat of the disease is less satisfactory than that given by the latter instrument.

The first objection is, I think, a serious one. For though we must all agree with Dr. Sims that delicacy and propriety, as well as the safety of the surgeon's character, require the presence of a third person whenever it can possibly be obtained on such occasions, still there are many cases in which no one qualified to render the surgeon the requisite assistance is

at hand. With regard to the second objection, all I can say is that whatever may be the case elsewhere, in this country at least very few decent women would, without some extreme necessity, which, in my opinion, does not exist in the cases now under consideration, submit themselves to a vaginal examination conducted in the manner described by Dr. Sims in his original directions for using the "duck-bill speculum." Even the greatly modified method of using this instrument now recommended by Dr. Sims in his valuable work on uterine surgery,* is, according to my experience, neither necessary nor expedient in these cases. The employment of the tenaculum hooked into the anterior lip of the os uteri, as directed by this eminent American surgeon, although now very generally practised in such cases, is, like several other points in modern gynaecological practice—

". . . a custom

More honoured in the breach than the observance."

It is true that the cervix uteri apparently possesses very little sensibility, and sometimes bears very rough handling, without any obvious ill effects. Yet it surely needs no argument to prove that it is not desirable to drag down the uterus towards the vulva by means of a steel hook fixed in an inflamed or ulcerated cervix. In other uterine diseases I have indeed often availed myself with advantage of this method of bringing the os uteri into view or the uterus within my reach in cases that could not have been treated otherwise. But, on the other hand, I have seen very unpleasant consequences from the indiscriminate practice of harpooning the cervix with a tenaculum for the purpose of remedying the surgeon's want of dexterity in bringing the os tincæ into view with the speculum.

Dr. Byrne of Brooklyn has recently published in the *American Journal of Obstetrics* a description of a new self-retaining speculum, or rather perinæum depressor, combining the advantages of the "duck-bill" and cylindrical specula, which, as far as can be judged from the inventor's account, promises to be a valuable instrument.

For all ordinary diagnostic purposes in such cases, I have found the ordinary glass-reflecting or Fergusson's speculum sufficient, and either that or Dr. Graily Hewitt's bivalve speculum was employed in at least nine-tenths of all the cases of ulceration that came under my observation.

IV.—*The Physical Signs of Ulceration of the Cervix Uteri.*

A digital examination will enable the experienced practitioner to diagnose a well-marked case of ulceration of the cervix uteri by the peculiar "soft velvety sensation" which, together with the patulous state

* *Clinical Notes on Uterine Disease.* By J. Marion Sims, M.D., p. 23. London. 1866.

of the os, was first pointed out by Dr. Bennett as characteristic of the disease, and which may readily be recognized by the touch. So conclusive are the tactile evidences of this condition of the os uteri that I have seen few out of the large number of these cases that presented themselves at the dispensary in which it was not possible to pronounce an opinion from a simple digital examination, even when only a slight degree of ulceration existed, unless it was confined to the cervical canal and had not affected the os or vaginal portion of the cervix. In fact, in the hands of an experienced practitioner, the speculum is chiefly required for therapeutic, and not so much as is generally supposed for diagnostic purposes. The os uteri is usually patulous and much lower down when ulcerated than is the case in its healthy condition, the whole cervix being tumefied and elongated. On making a digital examination we also find the parts to be more tender than natural, and the temperature is almost invariably raised. In short, all the characteristics of inflammatory action are present—“*tumor cum calore et dolore*”—as well as increased vascularity, evinced by the altered colour of the mucous membrane. Having ascertained by a digital investigation that the case is really one of ulceration, the surgeon may now proceed to make an ocular examination by the speculum. The lower part of the uterus will now generally be found congested and vascular looking, the lips of the os thickened, and usually one of them—commonly the anterior lip—longer and more prominent than the other. The vaginal portion of the cervix is coated with a thick glairy mucus issuing from the os and covering the softened hypertrophied mucous membrane, in which, when this discharge is washed away by syringing with tepid water, as it is so tenacious that it cannot be easily otherwise removed, the ulcerated surface may be detected with ease. These ulcers are irregular in form, and vary in extent from a mere speck-like depression in the mucous membrane to a deeply excavated sore. The superior fifth of the vagina at least always necessarily participates in the inflamed condition of the cervix, with which it is so closely identified by structure as well as by situation, and more or less vaginitis attends all cases of inflammation or ulceration of this part.

Ulcers of the os uteri are commonly very superficial, and in many cases are but mere abrasions of the mucous membrane. They generally commence just within the os tincæ, and extend ultimately to both lips, being, however, more marked on one side than the other. The os is usually very patulous, and we may frequently trace the ulceration through the open cervical canal into the body of the uterus.

The viscid glairy mucus, closely resembling the white of egg, which in these cases is seen issuing from the open os uteri, to which it adheres so closely as to be removed with much difficulty when we wish to examine the ulcerated surface, which is often completely concealed

from view by it is, as was pointed out by Dr. J. H. Bennett, whose researches have thrown so much light upon uterine diseases pathonomic of inflammation within the cervical canal. This mucus is secreted within the cervical canal by the glandulæ Nabothi, which in cases of cervical inflammation, and under no other circumstances, excepting during pregnancy, pour out a secretion by which the orifice of the uterus is hermetically sealed, and subsequent impregnation effectually prevented.

V.—*Causes of Chronic Endo-Metritis, Cervicitis, and Ulceration.*

In considering the causes of chronic inflammation and ulceration of the uterus, and especially of the cervix uteri, very undue importance is now-a-days attached to the local exciting causes of these affections, whilst the constitutional predisposing causes, which I consider as the *fons et origo malorum*, are disregarded. This matter is one of considerable practical importance, for if the views herein set forth as to the constitutional causation of local uterine diseases, attended by inflammation, ulceration, sterility, and other organic and functional derangements of the womb be right the treatment of these affections will be materially changed, improved, and simplified. The scrofulous diathesis is, in my opinion and according to my experience, one of the most common predisposing causes of the diseases that form the subject of the present essay. This fact, as far as I am aware, is completely ignored by all the recognized authorities on the subject, and my chief object in submitting this paper to the Obstetrical Society is to call attention to the connexion between struma and the most common forms of uterine disease, in the hope of thus contributing to a more satisfactory and rational plan of treating these complaints.

My attention was first called to this point by noticing that amongst the large number of patients suffering from chronic uterine affections, such as leucorrhœa, endo-metritis, inflammation, ulceration, or hypertrophy of the cervix uteri, ovarian pain, sterility, repeated miscarriages, derangements of menstruation, &c., that came under my observation at the dispensary for diseases of women attached to the Lying-in Hospital, a very considerable proportion were of well-marked scrofulous habit; in many instances they suffered from actual scrofulous disease of other parts, and in others I was able to trace the influence of a hereditary scrofulous taint.

In these cases the uterine symptoms, the character of the inflammation, the form of ulceration, and the nature of the discharge were all impressed with the scrofulous type. The constitutional condition of the patient being thus strumous, the local uterine disease is analogous to the scrofulous inflammation of the conjunctiva or phlyctenular ophthalmia, or still more resembles the chronic mucous nasal catarrh of strumous children, being attended by a similar tendency to produce excoriation of the mucous membrane, and by a somewhat similar glairy discharge. Many

of the symptoms of scrofulous inflammation are generally present in the cases under consideration; in them the inflammatory action is as insidious in its first encroachment, as chronic in its course, as obstinate in its duration, and as difficult to cure. Like all other forms of scrofulous inflammation that affecting the cervix uteri is peculiarly apt to lead to the formation of ulcerations of the characteristic strumous appearance, irregularly circular in shape, superficial in depth, pale and flabby in aspect, possessing little natural sensibility, but occasionally angry and irritable, tedious beyond patience when neglected or maltreated, and best cured by the treatment appropriate to other diseases of the same character.

In order to understand the peculiar susceptibility of the uterus to inflammatory and allied complaints, viz.:—Inflammation, acute as well as chronic; congestion, active and passive; ulceration, hyperæmia or hyperæsthesia—the anatomical and physiological peculiarities of this organ must be borne in mind; for it is only by studying vital actions in health, or physiology, that we can hope to succeed in studying morbid actions in disease or pathology. We must therefore keep in view, when considering this subject, the highly vascular structure of the womb, the formation of its veins, devoid as the cerebral sinuses of valves, and resembling them in their comparative size and number. The natural tendency to congestion of the uterus occasioned by menstruation must also be taken into account at the same time, as well as the possibility of these diseases being caused by arrested or defective involution after parturition, or, as is very frequently the case, by simple congestion, resulting from frequent pregnancies, or in newly married women from overstimulation of the parts. Inflammation of the cervix uteri leading to ulceration or hypertrophy is essentially a chronic complaint, and differs in this respect from inflammation of the body and fundus of the womb, which is more frequently of an acute character. That the cervix uteri is more liable to chronic inflammation than the superior part of the same organ is unquestionable, and admits of an easy explanation. For although there is no line of demarcation separating one portion of the womb from another, and limiting the diseases affecting each within certain boundaries, yet the situation of the cervix renders it more exposed to the exciting causes of inflammation, namely, mechanical irritation, or injury, to which may be added the natural tendency to congestion produced by the pendant situation and great vascularity of this organ.

Gout and rheumatism, or more commonly the gouty or rheumatic diathesis, must be recognized as amongst the occasional causes of chronic uterine disease. The importance of gout considered as a cause of these affections, was long since pointed out by the late Dr. Rigby. Neuralgia of the uterus is another form of chronic womb disease of constitutional origin, and to this or to gout or rheumatism must be referred the causation

of the "Irritable Uterus" described by Dr. Good in his classic essay, as of "Uterine Irritation," of which the late Dr. Addison has left so graphic a picture, and one which may still be read with much profit, though penned many years ago.

Syphilis is by no means an uncommon cause of uterine disease. But, contrary to a very prevalent opinion amongst dispensary patients at least, primary syphilitic ulcerations on the cervix uteri are extremely rare. In the few exceptional cases of this kind that I have seen there was generally also a chancre on the external parts, and even if there was not, the diagnosis was rendered easy by the well-defined, circular, excavated, and hard character of the sore, its greater sensibility, and the history of the case. These ulcerations require no further notice in this place, as they must be treated as ordinary chancres, wherever situated, should be.

Secondary syphilitic affections of the uterus are however comparatively frequent. Amongst the occasional manifestations of uterine secondary syphilitic taint may be mentioned leucorrhœa, or uterine catarrh; superficial abrasions of the mucous membrane; hypertrophy of the cervix, which in such cases presents a peculiar piebald colour, being in part of its normal aspect, and in part of a dull, congested, vitrious hue. All these symptoms however frequently occur without any syphilitic disease. In doubtful cases the diagnosis will be greatly aided by the history of the case; whether the patient has ever suffered from a primary sore on the external genitals, or from any suspicious cutaneous disease or form of ulcerated sore throat; or when none of these symptoms can be traced, by the fact that the patient has repeatedly aborted, or given birth to immature and putrid still-born children. Under such circumstances I should never hesitate to regard any obscure uterine disease as syphilitic, although I should be very cautious in imparting my diagnosis to any one whatever. I need not refer to the special treatment of these cases further than to say that if the general plan of treatment I adopt in all cases of inflammation and ulceration of the cervix uteri, which I am now about to give the details of, be carried out, there will be little need of any special treatment in the cases just referred to.

VI.—*The Various Forms of Ulceration of the Os and Cervix Uteri.*

Various forms of uterine ulceration have been described, such as "granular ulcers," "corroding ulcerations," "cockscorn granulations," "varicose," or "fungus ulcers," &c.; but these distinctions appear to me of little practical importance, and like some other recent additions to the already too complicated nomenclature of our present nosology, rather tend to embarrass the student than to assist the practitioner. It should be borne in mind, however, that simple ulcerations of the os and cervix may present either a granular or non-granular aspect.

Whatever form they may ultimately assume, all non-malignant ulcerations of the os and cervix uteri first manifest themselves as simple abrasions of the mucous membrane. In this stage of the disease there is merely an erosion of the epithelium of the affected part. The mucous papillæ of the ulcerated surface become hypertrophied and prominent, present a bright red colour, and are soft and velvety to the touch. The denuded surface whitens on the application of nitrate of silver, and there is a viscid secretion from the glands of the cervix. The adjoining mucous membrane is also inflamed, or at least congested. After a time the abrasion extends further into the subjacent tissues, the mucous villi are destroyed, and there is a corroded ulceration, with a distinct loss of substance evident on examination.

In the granular form of ulceration, which is only met with in chronic cases, the diseased surface presents a bright red raw appearance, from which the granulations project distinctly. After some time these granulations become pale and flabby, and assumes a fungous appearance. It has been asserted by M. Cazeaux and other writers, that a peculiar form of ulceration, marked by a hypertrophied fungoid condition of the mucous villi of the os uteri, frequently occurs during pregnancy, and in ordinary cases neither causes any inconvenience, nor requires any special treatment.

VII.—Symptoms of Chronic Uterine Inflammation.

The symptoms of chronic inflammation and ulceration of the cervix uteri are numerous, varying not only according to the extent of the disease, but still more according to the age and general condition of the patient, and hence little reliance can be placed on them until the nature of the case has been demonstrated by a vaginal examination. Holding, as I do, that ulceration of the os or cervix uteri is a result of pre-existing inflammation of a subacute, chronic, and frequently scrofulous, gouty, or syphilitic character, I shall here connect together my experience of the symptoms of chronic inflammation and ulceration, though these are generally considered apart. This inflammation, as has just been observed, is subacute in its form, and so insidious is its invasion, and gradual its progress, that, although it occasions numerous and important functional and structural changes and symptoms, these latter are, for the most part, so vague and uncertain, that until the complaint has passed from its first stage, that of active congestion or inflammation, into its second stage, that of ulceration, the nature of the case, nay, the very seat of the disease may remain undetected. Hence chronic inflammation attracts less attention, and is supposed to be less common than ulceration of the os uteri, merely because the latter is more easily recognized. The cervix uteri being congested or inflamed in these cases, the constitutional symptoms are to a great extent occasioned by the local hyperæmia. The patient complains

of pain in the back, across the lumbar region, and, following the course of the muscles which line the pelvis, down the thighs. This pain is generally worse after rest, so that she can hardly rise in the morning; and the sense of weakness in this part is such that the sufferer frequently explains it in the words—"I feel as if I have no back." A "bearing down" sensation in the pelvis and pain down the legs in the course of the crural nerves is also commonly produced in these cases by the pressure of the congested uterus, even when there is no displacement of the womb. The amount of local uterine pain occasioned by inflammation or ulceration of this part, even when extensive, is usually very slight. As a rule the patient complains more of a sense of heat and soreness, which is increased by sexual intercourse, rather than of any actual pain in the seat of the disease. She suffers from either a thin white or from a yellowish muco-purulent discharge, more or less profuse as the case may be, and this leucorrhœa, in the great majority of instances, is the symptom which first attracts the patient's attention to the uterine disease, and is the complaint for which she most commonly seeks advice.

The functions of the uterus are invariably disturbed in these cases. The menses are abnormal, occasionally they are attended by severe pain, in some instances they are diminished. Dr. Roe, in his excellent paper on the symptoms of uterine disease, read before this Society last session, says that dysmenorrhœa is more frequent than menorrhagia in these cases; but in the cases that have come under my observation on the contrary, the catamenia were more commonly too profuse, menorrhagia resulting from the local congestion and irritation, and in a large proportion of cases of this kind I have observed that menstruation recurred every third week, and lasted for six or seven days, whilst in the interval the patient was further weakened by the leucorrhœal discharge already spoken of.

Sterility almost always accompanies this disease, and as long as it exists to any serious extent the patient must remain barren. This fact, which I regard as one of great practical importance, is too generally ignored in practice. I have known cases in which patients were subjected to very heroic surgical treatment to overcome some supposed mechanical obstacle to impregnation, and who nevertheless remained childless, no attention having been paid to the true and most frequent cause of sterility, namely, the existence of chronic cervical inflammation, on the subsequent cure of which pregnancy has followed.

Ovarian inflammation, manifested by soreness, tumefaction, and occasionally burning pain in the ovarian region, is one of the most frequent consequences and accompaniments of endo-metritis. In these cases the inflammation extends from the uterus, along the fallopian tubes to the ovaries, and this to a great extent accounts for the fact I have just mentioned, that patients suffering from endo-metritis or endo-cervicitis,

are sterile for the time being. As a rule only one ovary, and that generally, although I know of no reason for it, the left ovary, was affected in the cases that came under my observation.

Vaginitis is present in almost every case of inflammation or ulceration of the cervix uteri, and occasionally pruritus of the pudendum is a most distressing complication of the disease.

The bladder soon becomes sympathetically affected, incontinency of urine and a scalding in micturition being amongst the most prominent symptoms of endo-cervicitis or ulceration. The bowels are generally confined in these cases, the rectum being frequently loaded with scybalæ, even when the patient persists in asserting that they are perfectly regular, and this constipation re-acting on the original disease adds to the uterine irritation.

The constitutional symptoms of chronic sub-inflammatory uterine disease are by no means pathognomic. As I have already stated, in a large number of these cases there are well-marked evidences of the strumous or gouty diathesis, and the sufferers are generally either of the lymphatic or bilious temperament. Hysteria in all its protean forms is one of the common consequences and symptoms of uterine inflammation or ulceration. There is generally some derangement of the digestive functions; the appetite is impaired, voracious or capricious as the case may be; the bowels are torpid; the intestines are distended by flatulency, which is especially troublesome after food, and a sick stomach especially in the morning is frequently complained of.

Cardalgia, palpitation and pain in the left submammary region is one of the most common symptoms of uterine disease. In such cases the patient generally seeks medical advice under the firm impression that she is suffering from heart disease, and will hardly allow any reference to the uterus as the seat of her complaint. In fact the majority of cases of supposed cardiac disease occurring in females, and especially when any evidence of hysteria can be detected may, *a priori*, be set down to chronic uterine inflammation, on the cure of which all the supposed cardiac symptoms will subside. The same observation applies to the chronic and other intense headaches, to which women suffering from inflammation or ulceration of the cervix uteri are peculiarly subject.

As the uterine disease progresses the patient's general health becomes more manifestly impaired. She loses flesh, becomes pale, sallow, or cachetic in aspect, her personal appearance being invariably, after some time, obviously altered for the worse, so much so that a woman who has long suffered from ulceration seldom retains any vestige of beauty. Her appetite is unhealthy, her tongue furred, and her breath offensive. She is weak and languid, and cannot take exercise without fatigue. The mind soon begins to sympathize with the body, and the patient becomes nervous, desponding, excitable, anxious, or irritable to the verge of insanity.

VIII.—*Treatment of Chronic Uterine Inflammation and Ulceration.*

The treatment of the complaints now under consideration may be divided into—first, the topical medication required by the widely varying forms of inflammation, hypertrophy, or ulceration that may be present in each case; and, secondly, the cure of the constitutional derangement, which always accompanies, and, as I believe, very frequently causes, the local disease. At the present time almost exclusive attention is paid to the former, the latter being as generally neglected. In proof of the little importance attached to the constitutional treatment of these cases it would be easy to quote authorities, but I will content myself with citing one of the most recent and ablest exponents of the modern school of gynecology—Dr. Atthill—who, in speaking of the treatment of inflammation of the cervix uteri, thus sums up what appears to be a prevailing view—“In my opinion,” he says, “medicines are useless in this disease.”^a For my own part, whilst I attach due importance to the local treatment of all uterine diseases, yet I am fully convinced, by experience, that the reason the cases we are now considering are generally so tedious and protracted in their cure is that the greatly improved local treatment now relied on is not assisted by proper constitutional remedies. I shall, therefore, in the first place, briefly allude to the constitutional treatment I consider necessary in these cases, for, as I have just stated, I regard chronic inflammation of the uterus and its results, including ulceration, as a consequence of a coincident constitutional disease, and, therefore, not to be cured by local treatment alone, except in those rare cases in which it is due to local mechanical irritation.

In accordance with the views already expressed as to the frequent connexion between chronic uterine complaints and the scrofulous diathesis, or other constitutional morbid conditions associated with gouty, or rheumatic, or, though less frequently, with syphilitic blood-poisoning, I would strongly urge the advisability of giving a fair trial to anti-strumous and other constitutional remedies and regimen in the cases now under consideration. I have seen numerous instances of what I regard as scrofulous ulceration of the cervix uteri thus cured without any local application beyond that of a little tincture of iodine and the use of vaginal injections. My experience of their use would lead me to recommend the simple preparations of iodine given in the small doses originally ordered by Lugol, such, for example, as the one-eighth of a grain of iodine with one-fourth of a grain iodide of potash, in preference to the larger doses of its compounds now generally prescribed. In cases where symptoms of anæmia predominate, the various combinations of steel with iodine are of singular benefit, all symptoms of uterine disease often disappearing under their

^a Clinical Lectures on the Diseases peculiar to Women. By Lombe Atthill, M.D., p. 161. Dublin. 1871.

use, without any local treatment whatever. Of all the remedies I have tried in these cases I have found no one so generally serviceable as a mixture of equal parts of cod-liver oil and syrup of iodide of iron.

Scrofula, although the most frequent, is, as has been just pointed out, by no means the only predisposing constitutional cause and accompaniment of the diseases we are discussing, and hence in a large number of cases we must have recourse to other constitutional treatment to alter that morbid state of the system which is the remote cause of the existing uterine congestion or inflammation. This must be effected by a modified antiphlogistic treatment, conjoined with rest, tonics, and sedatives, as well as the local use of baths or injections, caustics, astringents, counter-irritants, or local depletion, and, above all, the administration of any supposed specific remedy that may be indicated by the special requirements of each case. Thus, in cases of gouty origin, the preparations of colchicum and alkaline remedies, especially the mineral waters of Vichy, may be employed. In rheumatic uterine disease iodide of potash must be resorted to; in that dependent on constitutional syphilis, the remedies appropriate in other venereal affections must be tried, and in neuralgic uterine complaints our chief reliance must be placed on the preparations of quinine and iron.

As a rule, chronic inflammatory disease and its effects of the uterus, whether limited to the cervix or affecting the entire organ, when not of scrofulous origin, requires the administration of mercury, which is best given in the form of small doses of the perchloride, one twenty-fourth of a grain three times a day, in the tincture or infusion of bark. Notwithstanding the prevailing scepticism as to the efficacy of medicines in these chronic uterine affections, my experience of many cases has fully convinced me that although the local symptoms may subside for the time under purely local treatment, the patient is more quickly, as well as more permanently cured by the administration of constitutional remedies, such as those just referred to, whilst due attention is at the same time paid to the local treatment of the ulcerated or inflamed part.

The prevailing type of chronic uterine complaints, like that of all other general diseases of the present time, is essentially asthenic, and requires the administration of tonics in almost every instance, and more especially the preparations of steel, iodine and quinine combined when circumstances admit of it, with change of air and mineral waters.

The curative effects of change of climate and of mineral and thermal waters in cases of chronic uterine disease, as well as in other disorders, is a subject on which I am entitled to speak with some confidence, having given my attention to it during several years of travel and clinical observation in the health resorts of the Continent and the Mediterranean shores of Europe and Africa, as well as at the spas of Germany, France, and Italy. The accuracy of my views on these topics has, I think, been

best proved by the freedom with which they have been appropriated by some writers, who have spared my modesty by refraining from acknowledging their obligations to my works on *Change of Climate*,^a and *On the Spas*.^b

No class of remedies is so useful and so generally appropriate in all chronic uterine diseases of an inflammatory origin, and more particularly when producing ulceration or hypertrophy of the cervix uteri, as mineral and thermal waters used at their sources, and hence conjoined with change of climate. These maladies are peculiarly chronic in their course, and almost imperceptibly produce their ultimate result—namely, the causation of structural or organic mischief. The morbid change having been produced slowly, the action of our remedies must be gradual, for it is vain to hope that the administration of any remedy can suddenly undo the effect of years of disease. In mineral waters we have remedial agents of undoubted power, the action of which on the animal economy is generally so gradual that they require to be persevered in for long periods of time to produce their effects. Nor is the use of the waters the only service which a patient suffering from chronic uterine disease derives from a visit to some Continental spa. The journey to the foreign watering place involves a change of climate, of occupation, and of living. New scenes and places, as I have elsewhere observed, suggest new thoughts; the *atrabilis* of gloomy apprehension is purged away; the hysterical and oftentimes hypochondriacal victim of chronic uterine disease is induced to take her attention off her own morbid sensations, and ceasing to think on her symptoms, to a great extent they cease to trouble her. But entirely apart from the happy moral effect produced by a change from the routine drugging and dosing of an English valetudinarian lady's accustomed mode of life, which takes place when she leaves home for the gayer atmosphere of any of the German Bunnern or French *saie des eaux*, or even the comparatively sombre existence of an English watering place, the action of certain mineral and thermal waters on many of the diseases of women produced by inflammation or congestion of the womb is unquestionable.

Three distinct classes of mineral waters may be used in the treatment of chronic uterine complaints. The first are the iodated and bromated saline springs, the *Iod-und-Bromhaltige Kochsalzwasser*, as the Germans term such spas, as contain iodine and bromine, generally in the shape of bromide of manganese and iodide of sordium, dissolved in a muriated saline water. Springs of this kind are seldom thermal. The most important of these iodated or bromated spas are Wildeggen in Switzerland,

^a On Change of Climate: A Guide for Travellers in Pursuit of Health. By Thomas More Madden, M.D. London: 1st edition, 1864; 2nd edition, 1872.

^b The Spas of Germany, Switzerland, France, and Italy, in the Treatment of Chronic Diseases. By the same. London: 1st edition, 1865; 2nd edition, 1873.

Kreuznach on the Rhine, Adelheidsquelle in Bavaria, Hall in Austria, and Salzhausen in Hesse-Darmstadt. These waters stimulate the action of the mucous membranes, promote absorption, occasion ptyalism and diuresis, quicken the appetite, and act as powerful resolvents on all glandular enlargements. Hence their efficacy in the treatment of the diseases of women produced by chronic uterine enlargements and hypertrophy, the result of congestion or chronic inflammation of the womb; and especially in these cases of sterility which are supposed to be occasioned by hypertrophy of the cervix uteri.

The second class of mineral water applicable to the treatment of the chronic uterine diseases now under consideration are the chalybeates, both simple and saline. The former are those most resorted to by sufferers from chronic diseases of the womb, and are for the most part cold mineral springs containing the carbonate of the protoxide of iron dissolved in water containing more or less carbonic acid. Most of them hold a certain amount of manganese in solution. The saline chalybeates contain other saline ingredients in addition to the iron and carbonic acid. The action of the simple chalybeates is tonic and stimulant in proportion to their strength, exciting the nervous, circulating, and digestive functions, and at the same time improving the quality of the circulating fluid by increasing its fibrine and red corpuscles. Hence these springs are specially adapted for the treatment of cases of chronic ulceration of the cervix uteri and uterine or vaginal leucorrhœa, associated with anæmia as well as in the constitutional debility and loss of tone so frequently produced by, as well as conducive of, chronic uterine irritation, inflammation, congestion, or ulceration. Chalybeate waters also exercise a marked curative action in cases of hysteria dependent on these causes, as well as in certain instances of sterility. The principal simple chalybeate waters suitable for such cases on the Continent are Spä in Belgium, Pyrmount in Waldeck, Brüchenau in Bavaria, Schwalbach in the ex-Duchy of Nassau, and Driburg in Westphalia.

The saline chalybeate springs may also be used in various cases of chronic uterine diseases producing anæmia and complicated with abdominal and other enlargements, and, according to my experience, are particularly serviceable in the chronic uterine disorders so commonly caused in European women by tropical climates, and especially by long residence in India. These springs generally contain the salts of soda in combination with iron, and amongst them those most suitable for the cases we are now considering are the Stahlbrunnen of Homburg, Franzensbad in Bohemia, Bocklet in Bavaria, and at home Tunbridge Wells and Cheltenham.

Sulphurous mineral waters are the third class which I regard as applicable for the treatment of the uterine diseases above referred to. These springs derive their chief efficacy from sulphuretted hydrogen gas and

metallic sulphurets, generally of sodium or potassium. Their activity is mainly influenced by their temperature, being most powerful when this is highest. All thermal sulphurous waters are strongly stimulating, as they affect and act on the nervous, as well as on the vascular system, and can only be safely used in cases where there is no tendency to hæmorrhagic or other serious organic disease, where the patients' constitutional state is anæmic rather than plethoric, and where there is no danger of enkindling latent inflammation, and thus converting a chronic into an acute disease. The warm sulphurous waters that are available for suitable cases of chronic inflammation of the womb are Schinznach in Switzerland, Baden on the Limat, Aix-les-Bains, Eaux-Bonnes, and Amelie-les-Bains. Cold sulphurous waters may also be employed in some cases of chronic uterine inflammation or ulceration, and are far less stimulating than the thermal water of the same class. We possess in this country some of the most powerful cold sulphurous waters in Europe, namely, those of Swadlingbar, Lucan, and Lisdoonvarna, the latter is becoming to some extent known, the two former are almost disused, though all three might be advantageously used in many cases of chronic uterine disease, as well as Harrowgate, Moffat, or the more fashionable, because more remote, spas of Weilbach, Langenbrücken, and Wippfeld in Germany or Enghein-les-Baines near Paris.

Schinznach, the strongest sulphurous spa in Switzerland, is specially adapted for chronic uterine disorders connected with the scrofulous diathesis, in cases in which the constitutional condition of the patient is decidedly anæmic. This spa, a full account of which I have published in my work on the mineral waters of the Continent, is beautifully situated within half an hour's journey by railway from Basle, and presents many advantages for the temporary residence of anæmic ladies worn out by the excitement and fatigues of fashionable city life, and suffering from chronic inflammation or ulceration of the cervix uteri.

The Schinznach spa is thermal, the odour is strongly sulphurous, and the flavour I can only describe as a compound between the washings of a gun barrel and weak brine. It contains about fourteen grains of salts, principally chloride of sodium, sulphates of lime and magnesia, carbonate of magnesia, and chloride of ammonia in each pint. The most important ingredients of this water, in a remedial point of view, however, are the sulphuretted hydrogen, and carbonic acid gases, of each of which it contains so large an amount, that it cannot be used till some time after taken from the spring, so as to allow of the escape of a portion of these gases. In small doses it is tonic; in larger quantities it is a powerful stimulant, exciting the activity of the gastro-intestinal mucous membrane, increasing the excretions, and determining to the skin, on which a course of these baths frequently act so powerfully as to bring out a specific cutaneous eruption. This water is especially beneficial in scrofulous diseases,

whether affecting the external glandular system or attacking the mesentery or other internal structures. It is, however, to the undoubted influence of these baths and waters on chronic uterine diseases, attended by anæmia, chlorosis, and leucorrhœa, and resulting from chronic scrofulous inflammation, ulceration, or enlargement of the cervix uteri, that I desire to call attention. I believe we have in the proper internal administration of the Schinznach waters, conjoined with their employment as baths and in vaginal injections, a remedy of great therapeutic power in many of the chronic diseases of the womb. These waters, however, do not belong to the class of remedies which if they do no good can at least do no harm. On the contrary, they can only be safely employed in suitable cases, and, like all other strong sulphurous waters, and more especially all thermal sulphurous waters, always require the greatest caution in their use, being so powerfully stimulant as to be most unsuitable, and even most dangerous, for any patient of a plethoric habit, or who is threatened with any hæmorrhagic, cerebral, pulmonary, or cardiac disease.

The dose of this water is from one to three small glasses twice a day. It is also used in douch, vapour, and other baths, and the season lasts from May till September.

A very short distance from Schinznach is Wildegg, another and still more important spa for uterine cases. The mineral water of Wildegg rises through an artesian well, the supply furnished by which is so small as to furnish barely enough for exportation. This spa is one of the very few known iodated and bromated mineral springs, and is a most powerful remedial agent in congestion and hypertrophy of the cervix uteri, as well as in glandular scrofulous diseases, in which it is more generally employed. The Wildegg water must be used with great caution, in small doses of from two to four ounces, twice a day, as in larger doses it soon produces all the symptoms of iodism. Its internal use should be generally combined with a course of the Schinznach baths, and is administered in the same class of cases as that water.

In cases of uterine inflammation and ulceration cold saline hip baths are of great service, giving tone to the general system as well as to the seat of the disease, abating inflammatory action, diminishing hæmorrhagic and leucorrhœal discharges in some cases, and restoring the natural secretions in others.

Whenever uterine and ovarian dysmenorrhœa, pain, or any other evidence of inflammation is present there is no remedy of such universal applicability as the prolonged use of warm or tepid baths. Nature has given us a wide choice of such baths, suitable for almost every form of chronic inflammatory, uterine, and ovarian disease in the natural thermal springs which are found in almost every country. The waters which are used for this purpose are generally so feebly mineralized as to lead many to suppose that their effects are due to their mere temperature.

Be this as it may, however, the fact remains that tepid thermal waters exercise a remarkable sedative action on the nervous and vascular systems. Under their use the frequency of the pulse is diminished, pain insensibly disappears, and all nervous irritation is gradually allayed. Effects such as these point them out as especially suitable for cases of chronic uterine disease, leading, as is generally the case, to general as well as local hyperæmia, together with more or less hysteria or nervous irritability. Under these circumstances the effects of prolonged immersion of the body for hours together in water at the temperature of from 87° to 96°, or even 98°, is peculiarly sedative. The spas which are employed in this way, and from which I have seen most advantage in cases of uterine disease are those of Pfeffers, in Switzerland; Schlangenbad, in Nassau; Wildbad, in Wurtemberg; and Claudfontaine, in Belgium. To be of use these tepid thermal baths must be employed for long periods at a time, though it would be hard to persuade ladies of the present day to remain in their baths as long as was commonly the case at Pfeffers, when, as an old author assures us, they remained in the water for whole days together.—“*Multa dies noctesque thermis non egredientur; sed cibum simul et somnum in his capiunt.*”

Besides these, the thermal arseniated waters of Mont Dore and St. Nectaire, both in the volcanic district of Auvergne, may be used in the uterine disorders of scrofulous or neuralgic origin. The warm mineral waters of St. Sanveur, in the Eastern Pyrenees, which, in addition to their high temperature, contain a large amount of the peculiar pseudo-organic unctuous substance termed “glairine” or “baregine,” have a great and, I believe, well merited reputation in France in the treatment of scrofulous, rheumatic, and neuralgic affections, as well as in hysteria, leucorrhœa, and other complaints peculiar to women, resulting from chronic uterine disease.

It can hardly be necessary for me to observe that, although I attach so much importance to the constitutional treatment of chronic uterine maladies, which, I believe, is too generally overlooked at the present day, I am by no means insensible of the equal importance of conjoining efficient local treatment with the constitutional remedies indicated in such cases. Many of the principal recent improvements in the local treatment of uterine affections may be ascribed to Irish obstetric practitioners. Thus, for instance, to Dr. Kennedy, now President of the Dublin Obstetrical Society must be conceded the credit of applying the solid nitrate of silver within the cavity of the womb;* another of our former Presidents, Dr. Denham, subsequently improved upon this idea, and proved the safety of leaving a small piece of lunar caustic in the uterine cavity in

* Dr. Evory Kennedy on Inflammatory and Uterine Affections of the Uterus, &c., Dublin Quarterly Medical Journal, Vol. iii., 1847.

some cases of uterine hæmorrhage, a practice from which, in such cases, I have seen great benefit long before any account of this plan of treatment, as far as I am aware of, was published; and to our last President, Dr. Kidd,^a we are mainly indebted for the power of rapidly dilating the cervical canal by a number of sea-tangle tents, for the purpose of exploring the uterine cavity, and also for demonstrating the benefit which may be derived from the use of the fuming nitric acid in suitable cases of intra-uterine disease. It would be of little use to mention all the improvements that have been thus effected in uterine therapeutics, for a full account of which I would refer you to the writings of the late Dr. Beatty^b and those of Drs. Churchill,^c M'Clintock,^d Atthill,^e and other Irish obstetricians.

With regard to the local treatment of cases of chronic inflammation, ulceration, and hypertrophy of the cervix uteri, my observations will be very brief. I fully admit the great importance of local treatment in these cases. But to recapitulate the various local applications that have been recommended would be to give a list of remedies as long and as uninteresting as the "catalogue of the ships" in the *Iliad*, and therefore I shall merely mention the local applications from which I have seen most benefit derived in these cases.

Vaginal syringing, either cold or tepid, as may be most agreeable to the patient, was the first method of using any topical remedy in uterine diseases, and it still remains one of the most useful and indispensable. Various fluids have been proposed for this purpose. In cases of mere inflammation, or congestion without ulceration, or leucorrhœa, cold or tepid water, or infusion of chamomile, thrown up by a syphon vaginal syringe in quantities of a pint or two twice daily, is perhaps the most useful. Where an astringent is required the decoction of oak bark, with or without sulphate of alum, ℥i. to the pint, or a similar quantity of the compound powder of catechu mixed with boiling water, then strained, and used cold, or a very weak solution of the perchloride of iron will generally answer. If the discharge be offensive a dilute solution of permanganate of potash or of carbolic acid may be tried, and if pain be the prominent symptom a little liquor opii (℥i. ad. 0) may be resorted to.

These cases seldom come under observation till the disease has passed from the stage of congestion or inflammation into that of ulceration. If, however, the cervix be found congested on examination, whether

^a Dr. Kidd in the *Proceedings of the Dublin Obstetrical Society*, *Dublin Quarterly Medical Journal*.

^b Dr. T. Beatty, *Contributions to Medicine and Midwifery*.

^c Dr. Churchill, *On the Diseases of Women*. 5th edition.

^d Dr. M'Clintock, *Clinical Memoirs on Diseases of Women*.

^e Dr. Lombe Atthill, *Clinical Lectures on Diseases Peculiar to Women*. 2nd edition.

ulceration be present or not, it may be relieved by a few punctures with a sharp bistoury on the congested organ, or with less trouble to the patient, and generally equally effectually, by the application of a plug of wadding saturated with glycerine to the os and cervix uteri. This application, which was first suggested by M. Demarquay, and introduced into practice by Dr. Marion Sims, is of all others the most generally useful dressing that can be applied to an inflamed, ulcerated, hypertrophied, or congested cervix uteri. The introduction of a large ball of cotton saturated with glycerine, and provided with a properly adjusted string to facilitate its removal is invariably followed by a copious watery discharge or exudation of serum from the diseased surface. In this way it acts as a powerful depletant, and on the removal of the plug, which must be withdrawn within twenty-four hours, the part which may have previously been congested and angry-looking, the mucous membrane of the cervix, instead of its natural pink colour, being perhaps, in these cases, as red as the patient's petticoat, will be found pale and normal in colour; or if ulcerated, the abraded surface will appear perfectly clean and healthy-looking. It need hardly be observed that these effects are not permanent, but the repetition of the same application will for a long time continue to produce similar results, until either the remedy loses its power or the disease is cured.

In the majority of cases of simple ulceration of the os and cervix uteri a free application of a strong tincture of iodine twice a week is the best and most speedily curative local application that can be made. If this fails, as it sometimes will, the solid nitrate of silver, or a strong solution of this salt, may be similarly employed. Whenever there is any well grounded suspicion of the ulceration being of syphilitic origin the acid nitrate of mercury may be resorted to, but requires to be most cautiously used. In the severe cases of granular ulceration, extending, as is frequently the case, from the os through the patulous cervical canal to the body of the uterus, a single application of the fuming nitric acid, pressed firmly against the diseased surface, and passed through the cervix into the cavity of the uterus by a wire, thinly coated with cotton wadding dipped in the strong acid, when aided by proper constitutional treatment, effectually destroys the most angry and obstinate ulceration, leaving a clean, healthy, granulating surface as soon as the eschar has fallen, and seldom requires to be used a second time if properly applied.

With regard to the potassa cum calce, as advised by Dr. Tilt* as a caustic in these cases, my experience is very limited, but in the few cases in which I have seen it employed the results were not such as would induce me to recommend its use. Of the stronger caustic—the

* *A Handbook of Uterine Therapeutics.* By E. J. Tilt, M.D. 3rd Edition. London, 1868.

potassa fusa—advocated by Dr. J. H. Bennett and by the late Sir James Simpson, my experience leads me to coincide entirely with Dr. West, “that when adopted it is usually either out of place or superfluous.” I have seen great injury done in some cases which came under my observation, in which bolder practitioners had used caustic potash freely to cauterize an ulcerated os uteri, or to reduce an enlarged cervix. In two cases I was obliged to attempt to restore by dilatation the canal of the cervix which had been almost obliterated by the improper application of caustic potash, and one in which the vagina was thus occluded. Nor are these the only evils which may follow the employment of this heroic remedy. I have had to treat acute metritis produced in this way, and I have, therefore, not felt myself justified in using a remedy liable to produce such consequences, when other agents equally powerful, but less dangerous, could be employed.

Hypertrophy of the cervix uteri, considered as a cause of sterility, has attracted more attention than it deserves, and a variety of heroic surgical procedures have been proposed and are practised for its removal with this view. These include amputation of the enlarged cervix, as recommended by Dr. Marion Sims, the application of potassa fusa, or of the potassa cum calce, for the purpose of melting down the enlarged cervix, as advocated by Dr. J. H. Bennett, and the employment of the actual cautery, as practised by French surgeons. Now, in my opinion, all these procedures are, as a rule, not only useless but injurious in the majority of instances of hypertrophy of the cervix uteri, though each of them may be necessary and beneficial in extreme cases, and under exceptional circumstances. A much safer and, in some cases, very effectual mode of reducing the size of the hypertrophied part is the application of a solution of codine in glycerine.

I had prepared, to some extent, a table, from my note-book, showing the constitutional condition of the patients in a large number of cases of chronic inflammation and ulceration of the cervix, as well as the duration of the disease, the treatment adopted, and the result of each case; but I found it impossible to have it completed in time for this meeting, and hence, reluctantly, I have been obliged to omit it, rather than present it in an imperfect form. I may, however, observe that should I have another opportunity of submitting this table to the Society, it will be found to bear out my views as to the frequent constitutional origin of these maladies, and the benefits derived from co-joining constitutional remedies with local treatment in expediting their cure and preventing their recurrence.

The practical conclusions which I would venture to submit to the consideration of the learned Society which I have the honour of addressing, as the result of my experience of chronic uterine disease, are briefly as follows:—

1st.—That chronic inflammation of the uterus, and especially of the cervix uteri, producing hypertrophy and ulceration is, in the majority of cases, occasioned by constitutional causes, one of the most frequent of which is the scrofulous diathesis.

2nd.—That these diseases require constitutional as well as local treatment.

The constitutional treatment required has been already discussed, and I would again urge the benefits derivable, in these cases, from the use of the mineral and thermal water I have already spoken of.

In many cases of chronic uterine inflammation and ulceration of the cervix uteri, the inflammation may be subdued and the ulcer cicatrized by local applications alone. But whenever general treatment is not combined with the local measures depended upon, the inflammation subsides or the ulceration heals much more slowly, and the disease is far more liable to recur within a short time than is the case when suitable constitutional remedies are also resorted to.

I am aware that the opinions expressed in the foregoing pages as to the causes of chronic uterine inflammation and its results, as well as to the great importance of constitutional as well as of local treatment in these cases, differ *toto cælo* from the opinions of some of the most eminent modern writers on the subject. Nor would I now venture to come forward to controvert doctrines so generally adopted, were it not the views I hold on this subject have not been hastily formed, nor without some practical experience. For many years I have thought over the neglect of constitutional treatment in uterine disorders, and for some years I have had an ample opportunity of studying these diseases, and watching the comparative effects of different plans of treatment in the gynæcological wards and in the extern department of the great obstetric institution with which I was connected. I shall, therefore, conclude by repeating an observation I made in a former work, and which may, perhaps, account for the confidence with which I have expressed my ideas, believing them to be well founded on long and close clinical observation. Having in this way acquired some experience, and taken pains to gain accurate information, I have recorded the results of these observations, even when they did not coincide with the conclusions arrived at by other observers; for such differences of opinion but prove the wisdom of the first aphorism of the Father of medicine, that, in our art, "Experience is fallacious and judgment difficult."

DR. LALOR said, in reference to the principle laid down in Dr. Madden's paper, of the importance of constitutional treatment in the local affection of the uterus, which he had brought under the notice of the Society, he might mention a case that occurred to him some twenty-five years ago. It was a case of dysmenorrhœa, occurring in a strong healthy

looking young woman, the daughter of a farmer. Her family was rheumatic, and she herself had had one or two attacks of rheumatism. She consulted him for dysmenorrhœa; and, taking into consideration the rheumatic diathesis and the actual occurrence of rheumatism in the patient, it occurred to him that the local affection might arise from the rheumatism. He, therefore, ordered her iodide of potassium with a decoction of barks, and the first occurrence of menstruation afterwards was natural. This gave rise to the idea in his mind that it was as an anti-rheumatic that the iodide of potassium had acted, and the case made a strong impression on his mind in a practical point of view. He made these observations to invite the remarks of more experienced persons to the very important subject of the constitutional treatment of local diseases.

DR. GEORGE JOHNSTON said they must all feel much obliged to Dr. Madden for his paper on the diseases of the os and cervix uteri. No doubt, whatever, they might find those diseases existing in connexion with strumous diathesis; but he did not altogether agree with Dr. Madden's views on this point. In the generality of instances they found that ulceration, endo-metritis, and inflammation of the cervix, were connected not so much with scrofula as with derangement of the digestive organs. When a patient presented herself at the hospital the first question put to her was—"What do you complain of?" She replied, most probably, "I have a pain in my side." "Do you swell after taking food?"—"Yes." Are you constipated?"—"Yes." "Have you heart-burn?"—"Yes." "Have you palpitation?"—"Yes." In fact, all the symptoms of indigestion were present, unaccompanied, in many cases, with any scrofulous indications. As to the treatment, they generally attended to the state of the digestive organs, and got them into proper order; for, in the generality of the cases which came to the dispensary of the institution, they could not submit them to mercurial treatment, which they would do if they had them in the house. Constitutional treatment to strengthen the digestive organs was, therefore, adopted in those cases and with great benefit. At the same time they should attend to the local affection. He agreed with Dr. Madden as to the abuse of the speculum, but it was a most useful instrument, of course, to be used with judgment and in suitable cases. When it became requisite to use certain applications to the ulcerated parts, they could not well dispense with the speculum.

DR. JOHNSTON then exhibited an instrument which he used in place of the leech, to scarify the cervix, and which was readier and easier of application than the old system of leeching, and relieved the cervix considerably when congested and inflamed.

DR. STEWART agreed with the French writer who said, "the womb is

the woman, and the woman is the womb;" he believed that nearly all the diseases of women depended to a great extent on the uterus. He agreed both with Dr. Madden and Dr. Johnston in their views. He believed scrofula was the universal disease of the body, and he agreed with Dr. Johnston that the affections referred to arose from indigestion; but he should like to know what disease was not connected with the digestive organs. Brain disease and heart disease could be traced to the same fruitful cause, but in the present day with all that men eat and drank he could not see how the digestive organs could be healthy.

DR. KIDD said Dr. Madden had represented one side of the question, and the other side had been a little overlooked, and deserved more consideration than it had received. He believed, he might say, that they had two schools of gynæcologists since gynæcology had become a special study. There were those who looked upon disease of the uterus as a manifestation of a constitutional condition, and those who looked upon the constitutional condition as being the result of the local disease. They found that running through all the writings of the present century since the study of gynæcology had been renewed; for though these subjects were studied long ago, and some of the earliest writers had discussed them under the two heads, it was not until Recamier introduced the speculum at the beginning of the present century, that the matter occupied the attention of modern physicians. He believed the truth lay in the combination of the two views. They would see many cases of uterine disease, congestion of the uterus, and so called ulceration of the uterus, which was a very rare disease (they would see a condition which was called ulceration of the uterus, but it differed from ulceration elsewhere, inasmuch as there was no breach of continuity, and no loss of substance—two conditions he believed to be necessary to enter into the definition of ulceration), but they would see those so called ulcerations in cases where there was no scrofulous condition, no digestive derangement, and where any derangement that occurred might be fairly considered a reflex symptom, and not a cause of the disease. How often did they see a young girl passing out of her teens, who perhaps while she was menstruating had engaged in dancing, had gone out, walked in the cold or wet, met with a sudden check to menstruation, and as the result of that fell into bad health, which she concealed perhaps for months and even years. They would have her losing flesh, complaining of pain in the back, pain in the head, irritability of the bladder, pains in the knees, all of which were common forms of reflex pain from uterine disease. On inquiring into the matter they would find constant bearing down pains, leucorrhœal discharge, menstruation scanty and very painful or else profuse; and for all of these symptoms she had been under constitutional treatment for months, but was no better. In a case

of that sort it was a constant experience that by proper treatment directed to the womb all these symptoms were removed. It would be found that the uterus was enlarged, and painful on being touched, the os open, a mucous discharge coming from it, and a red areola around the orifice. It would be found that by rest, local depletion, and hot fomentations, these symptoms would be relieved, and an amount of benefit conferred in a short period that all the constitutional treatment used for months, and perhaps for years, had failed to give. That was a case in which the uterine condition was a local disease, and was the cause of the constitutional symptoms, and it could only be effectually dealt with by local treatment. Again, they would see a woman who had had a hard labour or a mal presentation, such as a case, for instance, that he saw yesterday of a woman who had some time ago a footling presentation; the face was turned to the pubes. The students did not know the proper way of delivering her. She was left some time with the body born and the head not through the pelvis, and they tried to pull it through with the face turned towards the pubes. There was contusion and laceration of the cervix, and the poor woman had gastric derangement ever since, with menorrhagia and leucorrhœa; pain in the left side and neuralgic pain in the top of the head—one of the most constant symptoms of uterine disease. She had gone on in this way treated by constitutional treatment, and not getting better. In that case there was congestion of the uterus, and inflammation of the lining membrane, and he had no doubt that treatment directed to the uterus would restore her in a short time to health. This was another example of uterine disease depending on a local cause, and giving rise to constitutional symptoms that could only be treated by properly directed local applications. He believed it was quite true that a scrofulous constitution would manifest itself in the uterus as well as in other organs, and where, although the uterine disease was set up by a local cause, they must, to treat it properly, adopt constitutional remedies; but at the same time they must not neglect local treatment. He did not think there was any physician, no matter to what branch of the profession he devoted himself, who would not feel that it was a matter of great importance to be able to combine local treatment with his constitutional treatment. This method was becoming every day more largely used, and even in the affection of organs in which until recently it was never attempted. Thus in affections of the lungs the application of pulverized fluids and vapours to the lung was of great benefit combined with constitutional treatment. No doubt a scrofulous constitution was a common cause of uterine disease, and in this country gout was also a cause of uterine affections. The late Dr. Rigby wrote a treatise on this subject, and mentioned various affections of the uterus that depended upon a gouty condition of the system. Chronic constitutional syphilis was also

a very common cause of uterine disease. He supposed it was quite possible that men who saw diseases and had them brought directly under the recognition of eye and finger, might allow themselves to overlook the constitutional cause of those diseases; and therefore Dr. Madden had done service in drawing their minds to this consideration; but they should not overlook the local condition too. Dr. Madden spoke in his paper of caustic potash, as recommended by Dr. Henry Bennett. It was possible it may have been used more frequently than was desirable, but he knew in his own experience there were many cases of uterine disease which could be cured in a few weeks by caustic potash, which, without its use, would run on for months without deriving benefit from any other treatment. We may often meet with a deposit of fibrinous matter in the substance of the uterine wall, that will go on from year to year, and keep the patient in bad health, and no amount of iodine or mineral waters will remove it, and yet, if an issue be made in the fibrinous deposit with caustic potash the large hard mass will melt down. It was not to be burned away; the object of the application was to set up an inflammatory process which would cause a softening, and absorption of the deposit, and in the course of a few weeks restored the uterus to a healthy condition. Therefore he did not agree with Dr. Madden when he spoke of the use of caustic potash as unscientific and unsurgical.

Dr. J. A. BYRNE said he did not take the same view as Dr. Madden had taken of endo-metritis or of inflammation of the cervical canal. He had had some experience in these matters, and the cases where he should be inclined to attribute them to scrofulous diathesis were very few. The cases were generally met with among women of the lower classes, who were badly clothed and badly fed, who bore children too rapidly, and proceeded to nurse them, affording the uterus no rest or repose. In many of these cases the patients were obliged to carry their children, which leads to a certain amount of malposition of the uterus. After a time a chronic irritation was set up, and ultimately an inflammation, which he did not regard as scrofulous. With regard to the treatment of these affections, he did not observe that Dr. Madden had mentioned a plan of treatment which he (Dr. Byrne) thought most beneficial, and that was perfect repose of the uterus, abstinence from marital intercourse. This was one of the reasons why patients suffering from these affections derived so much benefit from being admitted into the wards of an hospital. With regard to the speculum, while he would decry its unnecessary application, he thought it would be a dangerous practice to treat these affections without it. They could not satisfy themselves with precision of the nature of the affection, if they did not avail themselves of the speculum, and when carefully applied, in judicious hands, that instrument could not do any injury. The use of the speculum was very

different from the abuse of it. In a certain class of women, who were obliged to stand much in pursuit of their callings, a varicose condition of the cervix often occurred, and these cases derived great benefit by making the patient lie down. He agreed with Dr. Kidd, that much benefit was to be obtained from a strong application of caustic in these uterine affections. Within the last three weeks he had some patients from the country, and he found the greatest benefit resulting from an application of strong nitric acid to the interior of the cervix, together with local depletion and the external application of iodine. He remembered that the late Dr. Johns was a great advocate for local depletion and was in the habit of using a special instrument for the purpose something like a lancet with a number of spears. He did not think they could avail themselves very generally of the constitutional methods of treatment suggested by Dr. Madden, of sending their patients to continental watering places. In certain ranks of society that could be resorted to, and great benefit would be derived by the patient, not so much from the use of the waters as from change of scene and the incidents of travel. At the same time he was not to be understood as desecrating the use of these remedies whenever they were in a position to have recourse to them; but unless they were to send their patients to Portobello (about which there was a great discussion going on), he did not see how they would be able to derive much benefit from Dr. Madden's suggestion. He had no doubt that the administration of cod-liver oil with iron would be found of use in cases which they were certain depended on a strumous condition.

DR. HENRY KENNEDY said—If I were asked to state which side I took in the question before the meeting, I would say the constitutional: for I believe it to have very much more to do with these uterine affections than any local cause. My friend, Dr. Kidd, ingeniously argued that if a sudden stop were put to menstruation, serious symptoms, followed by bad health, would ensue; and here no doubt could exist of the existing cause. But I would ask any one was this not a very exceptional case, and not representing in any way the majority? I may observe that most, if not all, these uterine affections used to be cured long before the speculum came into use, and medicines that are now forgotten were then in great vogue. Of these I may mention the tincture of cantharides, or the drug itself, or bearing out what my friend, Dr. Lalor, alluded to, that is the rheumatic origin of some of these cases, the ammoniated tincture of guaiacum was largely and effectively used. One other medicine, too, seems to me of more general use than it gets—I mean arsenic, which, there can be no doubt, has a potent influence over many of the uterine affections which arise from a constitutional cause. Still it would be idle to say that local treatment

was not often useful, and, in some cases, indispensable. But this does not come within my province, and I shall only make one remark about it, that, under the sanction of the late Dr. Dwyer, the application of leeches to the sacrum gave the greatest possible relief in some cases of that terrible disease, cancer of the uterus, and this after opiates had failed.

DR. MORE MADDEN, in reply, said that perhaps he had been misunderstood with reference to the speculum. He never stated that it was unnecessary. No one was more assured of the necessity of using it in certain cases than he was. He had used it with great benefit in some of these cases, and hence he could not say the speculum was useless or local treatment unnecessary. They all knew that they had in local measures the most powerful means of combating local disease, but he contended that that local treatment ought to be aided by constitutional treatment, and he believed that constitutional treatment was but seldom resorted to in these cases. He agreed with Dr. Johnston as to the importance of employing measures to act on the digestive organs, and he had seen the greatest benefits derived from Dr. Johnston's practice when he was associated with him in the Rotunda Hospital. He had no doubt as to the possibility of uterine disease being occasionally cured in the manner suggested by Dr. Lalor by iodide of potassium, and he fully agreed with Dr. Kidd as to the importance of combining both modes of treatment. Dr. Byrne might not have heard him, but he certainly stated that to treat inflammation of the mouth of the womb effectually the woman should abstain from marital relations. He agreed with Dr. Kennedy's observations as to the older writers. They were too much neglected; their books were never referred to now, and any one who looked into a book five or six years old was supposed to be antiquated. He was sure that from the older writers they might derive many valuable hints, and they were indebted to Dr. Kennedy for calling attention to the subject.

The Society adjourned.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

DR. KIDD, President.

Bronzed Skin.—DR. DARBY exhibited a portion of bronzed skin which he had removed on the 20th September last from the body of a female who had been under his observation since 1847. When she last came into hospital she presented all the characteristic appearances of what was called "Addison's disease." The white of the eye was pearly, the inside of the lips mottled with brown spots, and the intermediate parts were anæmic. She wasted away with vomiting and diarrhoea. She had no disease of the lungs or other viscera.

The piece of skin which he exhibited had been removed from the right mammary region. A white patch might be observed on it, which was a cicatrix caused by his having in 1848 removed her breast for a very large tumour, which discharged a glairy fluid from seven or eight openings. The woman was reduced to the lowest ebb, so that his friend Dr. Symes and others strongly advised him against the operation. The woman, however, was resolute, and he removed the breast. She continued under his observation from that time up to the 21st September last.

He had brought with him the kidneys, with the supra-renal capsules, but could see no disease in either of them. The spleen was remarkably small. The tumour under which she suffered in 1848 was of a fibro-cystic character. She never had cancer, nor had she any relapse, but lived in good health till four years ago, when she changed colour, and began to decline. They all knew that the supra-renal capsules were often found to be disorganized and changed in their structure where there was no discoloration of the skin, or anything bordering on the disease described by Dr. Addison, and when in a case of that disease they found these bodies without any perceptible alteration, it tended to create a doubt in his mind of the correctness of the theory which associated bronzed skin with disease of the supra-renal capsules.—*March 30, 1872.*

Injury of the Spine.—DR. BENNETT exhibited a specimen of injury of the spine, taken from the body of a man, who had been admitted into the hospital of Sir Patrick Dun, on the 13th of last June, and who died four days afterwards. He had fallen, while carrying a sack of corn into a lighter, his head falling between the curved side of the vessel and the sack. He lost his senses for a short time, but became quite conscious a few minutes after his admission into the hospital, to which he was brought without any delay. There was little or no collapse or shock. He was paralysed, both as regards sensation and motion, from the

clavicles downwards, including the upper extremities. There was also priapism, and tympanitis was noticed on the second day.

The temperature of the limbs was about a degree higher than the standard of that of the head. He was very talkative, extremely anxious about himself, and quite cognizant of all that occurred about him. He had some difficulty of breathing, and slight pain in the neck. He was continually rolling his head from side to side, and the contrast was remarkably striking between the restless head and the motionless body.

The house surgeon having observed a depression, corresponding to the sixth cervical vertebra, formed the opinion that the column was broken at that level, but Dr. Bennett, finding in the paralytic condition of the upper extremities, evidence of the brachial plexus being involved, concluded that the chord was injured as high as the third cervical vertebra. The patient died on the fourth day after the occurrence of the accident, of œdema of the lungs.

Post-mortem examination.—On raising the œsophagus, pharynx, and trachea, and making what was known as "Bichat's section," scarcely a drop of blood was seen to have been effused. At the level of the junction of the sixth with the seventh cervical vertebra, there existed a distinct rupture of the anterior common ligament, and the body of the upper bone of the two mentioned was strained back considerably. Higher up, between the fifth and sixth vertebræ, there was seen a slight rupture of the ligament, and again at the level of the fibro-cartilage between the third and fourth bones of the column. There was a slight gritty feel in the substance separated from the bone, but nothing that could be called a distinct fracture.

The bones were then removed, from the seventh to the second cervical inclusive, and a vertical section of this portion carefully made. On examining the posterior aspect of the vertebræ, no trace of fracture could be discovered, except a minute grain of bone detached from the edge of the superior articular process of the fourth, similar to the osseous particle detached in front, as already mentioned, at the third articulation. These two osseous lesions did not differ much as to level.

Dr. Bennett was of opinion that the state of parts which he had described justified the opinion that the ruptures of the anterior ligament and the detachment of the minute particles of bone, were the result of a forcible and violent straining backwards, or extension of the cervical portion of the vertebral column.

The spinal cord, where it corresponded to the principal ruptures, was completely divided, but there was no effusion of blood in the sheath. Both these sections (as it were) seem to have been caused by the pressure of the laminae of the vertebræ upon the spinal marrow, resulting from the great degree to which the extension of the neck was carried.

At the time of the man's admission into the hospital, the reflex phenomena were well marked in the lower limbs.—*November 30, 1872.*

Disease of the Heart and Arteries.—DR. JAMES LITTLE said the morbid specimens he exhibited were illustrative of the pathology of atheroma and mechanism of cardiac dropsy, and were taken from the body of a man, sixty-two years of age, who had been under his observation for the past six weeks. He had been subject all his life to attacks of gout. He had one in January last, and when recovering from it he had an attack of bronchitis. In consequence of this he confined himself to the house for some weeks, but just as it was subsiding he had to go out and attend to his business, and from that time on he had a cough and a distressing feeling in the chest, which he spoke of as a burning sensation. In August last both legs became swollen, and soon afterwards he came under Dr. Little's care. His lower extremities were tensely cedematous. It required firm pressure to produce a pit, but when made it remained a considerable time. There was a small quantity of fluid in the abdomen; the thorax and upper extremities were entirely free from cedema. He had cough, and was unable to lie down owing to difficulty of breathing. The cough brought up a small quantity of very viscid mucus. There was fine crepitation over the lower third of the lungs posteriorly, presumably due to cedema of the lungs. The area of cardiac dulness was greatly increased, the impulse extremely feeble, and over the body of the ventricle a soft systolic murmur was occasionally audible. He was irritable, drowsy by day, and restless at night.

When he first came into hospital, he passed a considerable quantity of water. The secretion, however, soon became scanty, threw down lithates, and left a deep stain of colouring matter on the chamber; it never contained any albumen. The patient gradually sank into a state of insensibility and died, and thirty-six hours after death a *post-mortem* examination was made. There were then no signs of rigor mortis. The heart was found large and flaccid; its cavities were full of dark fluid blood. The ascending and thoracic aorta were of a deep vermilion colour. This was not caused by mere staining, for it persisted after repeated washing. The right femoral and brachial arteries presented the same appearance. He thought this case supported the doctrine now gaining ground—that atheromatous change in the blood vessels is due to a process allied to inflammation, and is not the result of a deposit from the blood. They found in these blood vessels all the stages of atheroma—in some places there was a buff coloured patch, in others the atheromatous new growth had raised the epithelium of the vessel so as to form a little tumour, in others the epithelium was washed away, and an atheromatous ulcer had been left, and at other spots, on the arterial surface of one sigmoid valve, for example, there was a calcareous plate. Besides this every portion of the

arterial tree which was examined, showed intense injection of the internal coat, which was of a deep vermilion hue. There was dilatation and hypertrophy of the ventricles, as had been inferred from the physical signs; their muscular fibre also showed a change which hypertrophied hearts occasionally present; it had undergone fatty degeneration, and when they sought for the cause of this degeneration, they found that the atheroma had involved the coronary arteries. His (Dr. Little's) reading of the whole case was this: for years past the man had some atheromatous disease of the blood vessels, this gave rise to hypertrophy of the ventricles; then in spring he had an inflammatory attack, which was prolonged and intensified by exposure; during its continuance there was increased disintegration of tissue, more work was thrown on the kidneys than they were able to perform, and the blood became more and more impure. This impure blood, acting as an irritant, set up inflammatory changes of a more acute kind in the coats of the blood vessels. The vessels specially affected by this were the coronary arteries, and, as a consequence, the muscular structure of the heart, deprived of its proper blood supply, underwent fatty degeneration.

There was, however, one other point in the case to which he wished to direct attention. Valvular disease and dilatation of the left ventricle are recognized causes of dropsy, yet it is known that persons may have valvular disease and dilatation, and yet not become anasaruous; in other words, these lesions alone are not sufficient to cause dropsy, there must be something superadded; in most cases, no doubt, that something is a watery state of the blood, but in other cases he thought atheroma of the arteries supplied the determining condition. Next to the force of the ventricle, the elastic recoil of the arterial tunics is the most important agent in keeping up the circulation; when that is destroyed greater labour is imposed on the ventricle, and by reason of the atheromatous roughnesses on the lining membrane of the vessels, an additional impediment, in the shape of augmented friction, is added, and, as a consequence, there is a greater tendency to stagnation in the capillaries and venous radicles. During the last few weeks of life this patient sat up in bed almost constantly; in this position his hands were kept dependent to a much greater extent than his lower limbs, yet they were not œdematous, while the feet and legs were slightly anasaruous, and after death it was found, that while the atheromatous change in the brachial artery was but slight, that in the femoral was advanced. If his (Dr. Little's) idea were correct, the heart, though dilated and weak, was sufficiently strong to push on the blood into the capillaries and venous radicles of the upper extremities, when it had the assistance of comparatively healthy arteries, but was not able to do so in the lower limbs in which the arteries were rigid and inelastic.—
November 30, 1872.

Endocarditis: Valvular Disease, the result of an Injury.—DR. M'SWINEY said, the morbid specimen he exhibited was a good example of a form of disease with which the members of that Society were already very familiar, viz., extensive disease of the left auriculo-ventricular opening of such a kind as to leave a permanent patency of the orifice, and to admit of the regurgitation of blood, followed by the train of pulmonary results which, ultimately, usually succeed to such a state. He should not, he said, have occupied the time of the meeting, were it not that he had a special object in submitting the present specimen to them. The man from whom the heart was taken was under observation but for forty-eight hours. He came into Jervis-street Hospital dying from pulmonary apoplexy, and in fact died in forty-eight hours after admission. At the autopsy the left auriculo-ventricular opening was found to be the subject of very considerable disease; there was a large mass of rough and irregular calcareous matter occupying half of the margin of the orifice. The valve was in a state of advanced disease, contracted, thickened, and adherent. He learned from the man that he had been in perfect health up to two years ago; that he never had been the subject of that one form of illness, which was almost always the precursor and cause of valvular disease of the heart—acute rheumatism, and that in fact he had not lain in bed for any sickness during his life. He was a very well formed, fine specimen of a strong man; he was six feet in height, and had been remarkable for his great muscular and athletic powers.

He said that he was engaged two years ago, being then in perfect health, in playing football. This game, as members were aware, was usually played in a very rough and energetic manner. At one time, whilst he was standing, not actually taking part in the game at the moment, one of the players, in a spurt of exuberant muscularity and reckless strength, rushed at him from behind, took him upon his shoulders, hoisted him into the air, and threw him with immense force on to the ground. He fell, of course, heavily, and was seriously hurt, and in fact completely stunned for a time. He remained insensible for a few minutes. About an hour or two afterwards he expectorated a considerable quantity of blood. From that hour to the time of his death he was in a state of sickness and ill health. He gradually wasted, and suffered from the usual consequences of heart disease—cough, disturbed breathing, bloody expectoration, and finally pulmonary apoplexy, from which he died. Now here, he (Dr. M'Swiney) believed, was an example of endocarditis directly due to injury from violence. It was, no doubt, extremely rare to have an example of this lesion due to injury inflicted upon or in the neighbourhood of the cardiac region presented to our notice, but still some such cases were on record, and were alluded to by Walsh, Flint, and others, and he (Dr. M'Swiney) submitted that this man, who had never been ill for an hour before he suffered a violent

arterial tree which was examined, showed intense injection of the internal coat, which was of a deep vermilion hue. There was dilatation and hypertrophy of the ventricles, as had been inferred from the physical signs; their muscular fibre also showed a change which hypertrophied hearts occasionally present; it had undergone fatty degeneration, when they sought for the cause of this degeneration, they found that the atheroma had involved the coronary arteries. His (Dr. Lee's) reading of the whole case was this: for years past the man had an atheromatous disease of the blood vessels, this gave rise to hypertrophy of the ventricles; then in spring he had an inflammatory attack, which was prolonged and intensified by exposure; during its continuance there was increased disintegration of tissue, more work was thrown on the kidneys than they were able to perform, and the blood became more and more impure. This impure blood, acting as an irritant, set up inflammatory changes of a more acute kind in the coats of the blood vessels. The vessels specially affected by this were the coronary arteries, and in consequence, the muscular structure of the heart, deprived of its normal blood supply, underwent fatty degeneration.

There was, however, one other point in the case to which he drew to direct attention. Valvular disease and dilatation of the left ventricle are recognized causes of dropsy, yet it is known that persons may have valvular disease and dilatation, and yet not become anasarca; in other words, these lesions alone are not sufficient to cause dropsy, there must be something superadded; in most cases, no doubt, that something is a watery state of the blood, but in other cases he thought atheroma of the arteries supplied the determining condition. Next to the force of the ventricle, the elastic recoil of the arterial tunics is the most important agent in keeping up the circulation; when that is destroyed great labour is imposed on the ventricle, and by reason of the atheromatous roughnesses on the lining membrane of the vessels, an additional impediment, in the shape of augmented friction, is added, and, as a consequence, there is a greater tendency to stagnation in the capillaries and venous radicles. During the last few weeks of life this patient sat up in bed almost constantly; in this position his hands were kept dependent to a much greater extent than his lower limbs, yet they were not oedematous, while the feet and legs were slightly anasarca, and after death it was found that while the atheromatous change in the brachiocephalic artery was but advanced, that in the femoral was advanced. If his (Dr. Lee's) theory were correct, the heart, though dilated and weak, was sufficient to pump the blood into the capillaries and venous radicles, and it had the assistance of comparatively healthy arteries to do so in the lower limbs in which the atheroma was advanced.

November 30, 1872.

Case.

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that all the symptoms were of a more aggravated character. On the evening of the 19th his condition had undergone some improvement, and he declared himself much better. He was seen at ten and eleven o'clock respectively by the sisters in attendance, when he seemed better, but on the following morning he was found quite paralysed on the right side, and unable to utter a word. When Dr. Hayden saw him at eleven o'clock there was complete paralysis as to motion of the upper and lower extremity of the right side, but sensibility was rather exalted. The features were slightly drawn to the left side. The pupils were normal and equal, and there was no strabismus. He perfectly understood what was said to him, and in the effort to reply, his features were drawn to the left; he could not utter a word save "yes" and "no," and these very indistinctly. The pulse was rapid, and the skin hot, and the signs of accumulating mucus in the air passages existed. He either would not or could not protrude his tongue when requested to do so, but the deglutition liquids was unaffected. The tactile and acoustic signs at the precordium were those previously noted. He died in the course of that evening (20th), evidently from the bronchitis.

The brain was large, and nowhere softened; the arteries at the base were empty, except the left anterior and middle cerebral, both of which were plugged to distension with small and irregular particles of solid fibrin. The terminal portion of the carotid, at the proximal side of the obstruction, was empty, but the branches arising from both cerebrals at its distal side were full of dark liquid blood. The bronchial tubes throughout were congested, and contained much blood-stained, frothy mucus. The heart was somewhat enlarged and globular; the right chambers contained some dark soft coagulum and a few detached masses of grey fibrin. The former extended into the pulmonary artery, and was there coated with a film of fibrin. The right ventricle was dilated and thinned; the left auricle dilated and thickened; the left ventricle was likewise dilated and hypertrophied, and its cavity rounded. The mitral orifice was constricted in a high degree, barely admitting the point of the index finger. It was of button-hole figure, its edges were smooth and thick, and formed by the edges of the valve segments, which were elsewhere firmly united. To the antero right segment a mass of solid fibrin, as large as a small playing marble, perfectly round, but rough and shreddy on the surface, was attached. This mass involved about one-half the depth of the valve in its substance, and when the valve was displaced by the stream of blood entering from the auricle it must have been thrown athwart the orifice of the aorta, and was, no doubt, the source whence the cerebral emboli were derived, or washed away by the passing stream of blood.

The aorta was reduced in size, and its valves were thickened, corrugated, and inadequate to close the orifice.

Dr. Hayden remarked, in reference to this case, that it illustrated—

firstly, the diagnostic value of presystolic fremitus and murmur as signs of mitral contraction; secondly, the usual absence of the history of rheumatism in connexion with this lesion; thirdly, the precipitation of fibrin from the blood whilst still in circulation, from the twofold cause of partial stasis and roughened endo-cardial surface; and lastly, the detachment by the circulating blood of a certain amount of detritus from the surface of the thrombus so formed, its impaction in the left, middle, and anterior cerebral arteries, by which the left motor centre and Broca's region of language were deprived of blood, and *right* hemiplegia and aphasia were produced.—*November 30, 1872.*

Pericarditis.—DR. LYONS said the specimens he exhibited had been taken from the body of a man admitted three weeks previously to the Hardwicke Hospital, where he was sent apparently labouring under fever. He had pyrexial symptoms in a certain degree, but not true fever. He had constant sweatings and chills; the pulse varied from 130 to 140, and was extremely feeble. He also suffered from paronychia of the index finger of the left hand, to open which two attempts had been made several days before his admission. Dr. Lyons' attention was called to this by the patient as apparently a trivial adjunct to the disease. In the course of a couple of days it became necessary to open the paronychia lower down, and a considerable quantity of pus escaped. He experienced great relief, and was apparently going on well, when he began to suffer from extreme difficulty of breathing. On making a careful stethoscopic examination, it was plain that he was labouring under pericarditis. The area of dulness was increased, and there was a peculiar kind of sound, difficult to describe, but which gave an idea of the heart flopping about in some soft, semi-fluid material. The sweats continued, the pulse rose higher and higher. He complained of a swelling of the glands in the right groin, which were tender to the touch, and he also complained of a good deal of pain in the neighbourhood of the hip-joint. He had made some reference to this symptom on his admission to hospital, but it was not till several days afterwards that he directed attention to the pain in the groin, and then to the pain in the right hip, as being at all of a severe character. In the progress of the case the last phalanx became necrosed, and it was evident that there was no reparative power in any part of the finger. The man died on the twelfth day after admission.

The *post-mortem* examination, which was made very carefully by his clinical clerk, revealed the following state of things:—The sac of the pericardium was enormously distended, and contained fully a pint and a half of purulent fluid, and on allowing that to escape, they found a quantity of soft flaky lymph on the surfaces of the heart and pericardium. It was of such a soft character that it could not produce

any distinct friction sound, and he had no doubt the peculiar cardiac sound which had been heard during life, was caused by the rubbing together of these opposed surfaces coated with lymph of a butter-like consistence. It had but little adhesive character, and could be stripped off with ease. It was an example of that low form of purulent pericarditis which occurred where a general pyæmic condition was present in the system. There was also found a large abscess, containing a pint of pus, occupying the site of the right psoas muscle, having destroyed a considerable portion of its muscular structure. This, no doubt, was the cause of the glandular irritation in the groin, and the pain complained of by the patient in the neighbourhood of the hip-joint.

As the early and complete history of this case was not fully known, it was difficult, if not impossible, to say what was the starting point of this morbid condition. The man was a pensioner; he had seen a good deal of foreign service, by which his constitution was much run down, and he had still further weakened it by a free imbibition of stimulants. It was just probable that the paronychia might have been the starting point of the whole train of symptoms; that the man's constitution had got into a pyæmic state from absorption of pus from this disease; and that thus pericarditis, the abscess, the glandular swelling, and the pain in the hip-joint had been produced from a primary cause of apparently so trivial a character.—*November 30, 1872.*

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OF

MEDICAL SCIENCE.

APRIL 1, 1873.

PART I.

ORIGINAL COMMUNICATIONS.

ART. VIII.—*Liquor Sodæ Chloratæ: A few Observations upon its Effects.* By ROBERT T. COOPER, M.D., T.C.D., Southampton.

THE *Liqueur de Larbarraque*, as the liquor sodæ chloratæ is sometimes called, is a preparation that belongs to a rapidly increasing list of favourite remedial agents—namely, such as possess the property of arresting or otherwise modifying the putrefactive process, and rivalled by such drugs as carbolic acid, chloride of aluminium, permanganate of potash, it runs the risk of being supplanted without sufficient reason. On the contrary, an extensive acquaintance with the therapeutic effects of chlorinated soda has convinced me that it possesses most important properties. It is very generally admitted that most salts of chlorine are valuable disinfecting and deodorizing agents when applied to the seat of foetid discharges, and that their effects are due principally and proportionately to the facility with which they part with their chlorine. That this does not fully account for the beneficial effects of chlorinated soda, we are much inclined to think; at all events, if it does, chlorine must henceforth be looked upon as a much more valuable drug than it has been. Chlorinated soda has long enjoyed a favourable reputation as an application for suppurating and cancerous ulcerations, leucorrhœal and other catarrhal discharges, but I am not aware that it is generally supposed that, taken internally, it has a plainly marked stimulating effect upon

relaxed states of the uterine and peri-uterine tissues; that it gives tone to the weakened utero-sacral ligaments, increases vaginal contraction, removes the bearing down and tendency to prolapse, diminishes congestion of the neck of the womb, thereby lessening very considerably leucorrhœal^a and menorrhagic discharges; and yet this is what we claim for it, and more than this, that along with the removal of the original affection, the casual congestion, comes agreeably to the *tolle causam* principle, the more or less entire subsidence of sympathetic disturbances, the sacral, ovarian, rectal and vesical distress, the hypochondrial tumefaction, the gastric and precordial sinkings, the sub-mammary stitch, chest pains, and headaches, a pretentious assertion certainly, but one that an extensive experience with it more than justifies. Nor need we hesitate to speak freely, as the condition of system for which we recommend it is every day met with, and every practitioner, even specialists in departments of medicine remote from uterine, must be aware that their daily concourse with disease brings them into contact with affections complicated with and modified by this more than common uterine enfeeblement.

Like most efficacious remedies, the liquor sodæ chloratæ can disturb as well as control the nervous system, and therefore it requires to be administered in doses graduated according to the nervous susceptibilities of each invalid and the nature of the affection under which they suffer. The dose given in Squire's *Companion to the Pharmacopœia*, namely, ten to twenty drops, is much too large for our present purpose; the uterine maladies calling for its employment can be controlled, as we have confirmed by experience, with much smaller quantities. The method of administration we recommend is to put two or three drops of the ordinary commercial solution in half a tumblerful of water, and direct a dessert spoonful of this to be taken occasionally. This will be found quite sufficient, and in susceptible persons disturbing effects even may be noticed. Should symptoms of the medicine disagreeing occur, such as rapid emaciation and influenza-like debility, which is one of its most uniformly unfavourable symptoms, it ought to be forthwith discontinued, and the patient advised to remain without medicine for some time.

As this paper is chiefly tentative, I refrain from enlarging upon what I feel sure is a very important matter, my object being to

^a It does not, nor does any internal remedy that we know of, cure a chronic leucorrhœa.

elicit the teachings of others' experience rather than to ventilate my own; this only by way of explanation: I by no means would be understood as asserting that chlorinated soda can cure without exception every case of uterine congestion; it can cure a great many—more than any other drug it has been my good fortune to come across.

In ulceration of the neck of the womb, hypochlorite of soda, by reducing congestion, gives great relief, but I leave for others to determine what is its relative value in comparison with the favoured method of treatment by stimulating applications. We do not wish to express any opinion whatever upon this particular, feeling that in such quarters we stand upon holy ground; but we think we may allowably infer that, judging from its influence over ulcerations in other parts of the body, and especially over ulcers of the legs in women subject to uterine disease, its place as a constitutional remedy in the hands of the gynæcologist must in time prove to be an important one.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Practical Treatise on the Diseases of Women. By T. GAILLARD THOMAS, M.D., &c. Third edition. Enlarged and thoroughly revised. Philadelphia: Henry C. Lea. 1872. Pp. 784.

THIS is an American work, and has already reached the third edition in its native country—an honour that it well deserves, not only by the breadth and philosophical tone of its teaching, but by the accuracy and completeness of its details.

So much attention has of late been paid to the diseases peculiar to women; so much has been written on the subject, and so much controversy has arisen as to various points in the pathology and treatment of these diseases, that the student who would enter on their study without some sketch or summary of what has hitherto been done, would soon find himself involved in difficulties that would take more time and labour to overcome than are at the disposal of most of us. Dr. Thomas has then, we think, done wisely in prefacing his very excellent work by a history of gynæcology; and we propose, in the present notice, to lay before our readers a brief sketch of this history.

For all practical purposes we may, perhaps, look on the study of gynæcology as having commenced at the beginning of the present century, when Récamier re-introduced, or, as is sometimes said, discovered the speculum, but, in fact, the diseases of women formed a special branch of medical study from the earliest periods, of which we have record, and many of the discoveries and inventions of which we sometimes boast in the present day, are described or foreshadowed in the works of some of our oldest writers. Dr. Thomas specifies as examples of these the use of the uterine sound, sponge tents, dilatation of the constricted cervix, and even the speculum itself. We may add to the list the application of strong nitric acid to the interior of the uterus—a mode of treatment described in our own pages a few years ago by Dr. Kidd, as having been adopted by him in a case where crop after crop of

small-sized polypi, requiring removal, had grown in the interior of the uterus. At the time Dr. Kidd looked on this as a new and heroic mode of treatment, only to be excused by the exigencies of the case, but he has since stated, at a meeting of the Dublin Obstetrical Society, that the same mode of treatment is described by Ambrose Paré, as having been safely and successfully used in apparently analogous cases, described as wart-like growths in the interior of the uterus. From what we now know of it, it seems not at all unlikely that nitric acid was also used for the treatment of many other conditions of the uterus, as it has already come to be in Dublin.

Dr. Thomas traces the history of gynecology from the first indications found among the hieroglyphics of the early Egyptians down to the present day. We must refer to the work itself for what he says as to this early history, and come on to more modern times to see how he treats of some of our more recent controversies.

Soon after the revival of the study, gynecologists ranged themselves into two parties, which even yet have not fully coalesced. In England the feeling was strongly in favour of regarding local uterine disorder as the result and not the cause of the concomitant constitutional derangement, while in France the uterine disease was viewed as the main element and the general condition as dependent upon and resulting from it. Dr. Thomas adopts in a great degree the latter view, as, indeed, all who study disease at the bedside, and without preconceived ideas, must do. At the same time, he recognizes the great influence the constitutional condition may have in both initiating and modifying the local disease; but many other questions have arisen in connexion with this primary one, which, for the sake of clearness and accuracy, must be discussed with it. These Dr. Thomas states as follows:—

“1st. That inflammation is the starting point of most of the affections of the uterus, and that a large number of evils follow this morbid state as results.

“2nd. That uterine disorder is dependent upon a constitutional derangement, and would yield without other treatment than that directed to the removal of the general condition.

“3rd. The view of Dr. Bennet, which is similar to the first-mentioned, with this additional point, that metritis generally limits itself to the neck, and only exceptionally affects the body.

“4th. The view of Dr. Tyler Smith, that leucorrhœa arising from

glandular inflammation in the cervix is the cause of granular degeneration of this part, and of subsequent engorgement.

"5th. The view that uterine disorders often, if not generally, commence in displacement, which is a primary and not a secondary condition, and that to relieve the train of morbid symptoms, this, its exciting cause, should be first removed.

"6th. The view that uterine disorder is commonly the result of ovarian inflammation, which reacting on the womb is the prime mover, in many cases, of its morbid states."

The first of these theories is the oldest on record, being that of the earliest writers. It was generally accepted in France at the beginning of the present century, and upheld by Lisfranc and Récamier, and taught in Great Britain by Dr. Henry Bennett, of London, who, after Simpson, was one of the earliest and most prominent teachers of modern gynecology in Great Britain. In his work first published in 1845 Dr. Bennett insisted on the following points:—1. That inflammation is the *primum mobile* in uterine affections, and from it follow, as results, displacements, ulcerations, and affections of the appendages. 2. That menstrual troubles and leucorrhœa are merely symptoms of this morbid state. 3. That in the vast majority of cases inflammatory action will be found to confine itself to the cervical canal, and not to affect the cavity of the body. 4. The propriety of attacking the disease in its *habitat* by strong caustics.

These doctrines were so earnestly and vigorously advocated by Dr. Bennett as to gain for them a very general acceptance, but many were found to oppose them, and a warm controversy arose between Dr. Bennett, on the one side, and Dr. Robert Lee, Dr. West, and Dr. Tyler Smith, on the other.

Though the controversy led to no definite result, but rather caused Dr. Bennett's views to be more generally accepted, we believe that most practitioners, even those who like Dr. Thomas cordially adopted them in the first instance, will now agree with him in saying that careful investigation at the bedside necessitates a material change of opinion. In the early editions of his work Dr. Thomas adopted Dr. Bennett's views, but his opinion has now altered, both as to the pathology and treatment of uterine disease; not that he looks on Dr. Bennett's belief that inflammation is the great cause as an error in itself, but rather as a principle too exclusively adopted, and the treatment founded on it as too severe,

and the consequent neglect of artificial support for the displaced uterus too decided.

There are many circumstances, Dr. Thomas admits, that tend to support the idea that uterine disorder is dependent on constitutional derangement, and would yield without other treatment than that directed to the removal of the general condition, but there are three facts, he says, which lead to a distrust of the doctrine—1st. The fact that uterine disease and constitutional derangement existing together, a cure can rarely be effected by general means *alone*; 2nd. That the uterine state being removed the general condition is at once improved; and 3rd. That those general conditions that prostrate the vital forces to the last degree, as for instance tuberculosis, uræmia, scurvy, leucocythemia, &c., destroy life without ever showing, unless as an exception to a rule, the local disease as a consequence of a constitutional malady. We confess we do not just see the force of this last argument, and we think our author more correct when he asserts that the constitutional depreciation of a woman will sometimes prove a predisposing cause of local disease, but that even under these circumstances the local disease requires local as well as general treatment.

The theory of Dr. Tyler Smith, that cervical leucorrhœa is, in the majority of cases, when a morbid condition of the os and cervix is found the most essential part of the disorder, is dismissed by Dr. Thomas, with the very obvious remark, that the leucorrhœa is only a symptom of cervical endo-metritis.

To the theory of displacement, as the cause of uterine disease, Dr. Thomas attaches more importance, but he is far from adopting it. We are mainly indebted to Simpson's paper, published in this Journal in 1868, for our knowledge of displacements of the uterus, but he never attached as much importance to them as did some of his followers, especially Velpeau, to whom Dr. Thomas refers as the most uncompromising advocate of the influence of uterine displacements. In the year 1854, a discussion, which soon assumed extensive proportions, and elicited great warmth, arose in the Academy of Medicine of Paris with reference to the treatment of displacements. M. Velpeau declared in this discussion that the majority of the women treated for other affections of the uterus have only displacements, and affirmed that, eighteen times out of twenty, patients suffering from disease of the womb, or of some other part of this region, and in whom inflammation had been diagnosed, were affected by displacements. We shall see further on

that Dr. Thomas takes a much more moderate view of the true value of displacements. To the remaining theory, or that which asserts that uterine disorder is commonly the result of ovarian inflammation, Dr. Thomas does not, it appears to us, attach sufficient importance. He has, he says, very often met with cases of simple uncomplicated uterine disease in which the patient has been treated for ovarian disorder. Frequently, he says, this theory is used as a cloak for ignorance, the physician fixing on it from his inability to determine the real pathological features of the case. At other times sensitiveness over the ovaries with enlargement, is regarded, by capable men, as producing a series of evils, no special attention being paid to co-existing uterine disease, which is viewed merely as a complication. He admits, however, that ovarian inflammation which is clearly diagnosticable gives rise to many of the symptoms of uterine disease, but, under these circumstances, a carefully-made differential diagnosis will generally settle the point. Nor is it less certain that uterine disease very frequently produces sympathetic troubles in the ovaries, resulting in great sensitiveness on pressure, and sometimes enlargement. As, however, in this case no treatment directed to the ovaries will affect existing uterine disease, while curing the latter will generally remove the ovarian disorder, it appears to Dr. Thomas we must conclude that the ovarian disease is the result of the uterine. For our own part, we believe that the ovary, as the most important organ of the female system, is often the one to take on the initiative in the chain of morbid actions, and that the uterine disease follows. At the same time we must admit that the uterus is the organ most exposed to the exciting causes of disease, and is, consequently, the organ most liable to suffer and most frequently affected.

Having thus followed Dr. Thomas through this portion of his historical sketch, we shall not enter on the other portions of it, but pass on to Chapter XII., which is headed "General Considerations upon Uterine Pathology and Treatment," from which we make the following extract, as a fair statement of his own opinion on the subjects under consideration:—

"One set of authorities, unfortunately I may say one 'sect,' has entertained the view that inflammation of the parenchyma plays the important part of moving cause in uterine disorders; another that displacements of the uterus do so; another that the chief trouble consists in an irritation or hyperæsthesia in the uterine nerves; another that catarrhal inflammation of the uterine mucous membrane, is the origin of most of its disorders;

while still another attributes to the inefficient restoration of the uterus after the structural changes, due to utero-gestation, the most important rôle. To one who calmly and dispassionately considers the subject, not in the study, but by the bedside, and who goes to it with a mind free from prejudice, and eager for the discovery of truth, it appears to me that it must in time become evident that that truth lies not in any *one* of these theories, but is to be found to a certain extent in each. To any one who endeavours to reconcile the conflicting views of others, the appearance of presumption is very apt to attach itself. I trust that it will not do so here. In expressing the views which I do, I am merely candidly striving to state what has impressed itself upon my own mind from absolute observation. These are my own views honestly arrived at and fairly stated, and I feel sure that I have generally succeeded where I have had clinical opportunities of demonstrating them, in convincing others that they have a basis in truth. Let each reader test the statements here made at the bedside, and not too hastily and from theoretical reasoning alone, conclude that the author is making a politic attempt in *mediis res tutissimum ire*.

“That the uterus should perform its functions sufficiently and naturally it is essential, 1st, that its innervation and circulation should be normal; 2nd, that its structure should be unaltered in character and proportions; and 3rd, that no decided and permanent change should have occurred in its position. An abnormal state developing in connexion with any one of these essential conditions, may derange the functional powers of this important viscus, and demonstrate itself by symptoms which produce greater or less discomfort to the woman. When, as very often happens, the first evil produces others, until at last all three conditions are interfered with, the gravity of the symptoms increases with simultaneous increase in their number and variety. Sometimes the first link in the chain of morbid action is an altered condition of the nerves governing circulation, some general or local condition reflecting itself upon these regulators of nutrition; as a consequence, an afflux of blood takes place to the uterine mucous membrane, and its vessels become distended, and in time dilated. This lasts for a variable time, when the second link is furnished in this manner: an excessive degree of nutrition is supplied to the subjacent connective or areolar tissue of the organ, and its size and weight increase. Then the third link rapidly develops itself. The uterus now being heavier than normal, its natural and hitherto sufficient supports are insufficient for its maintenance in position, and it descends in the pelvis, so as sometimes to alter the direction of its axis, and protrude between the labia majora; at other times its axis is not changed in its descent, and then the cervix striking against the curved surface of the sacrum, is bent forwards so as to offer an obstruction to the escape of menstrual blood; at others the fundus falls forwards, laterally, or backwards, either bending upon the neck, or by its displacement forcing this

part out of position likewise. Then appears as symptoms of this three-fold disturbance, leucorrhœa, backache, dysmenorrhœa, difficulty in locomotion, and the long list of discomforts to which women thus affected are liable.

"This, however, is by no means always the sequence of events. Sometimes the uterus enlarged by utero-gestation does not return to its original small size, but remaining large and heavy, it falls from its place in consequence, and this disorder of position results in subsequent disorder in the other two conditions which I have stated are essential to health—normal innervation and circulation, and unaltered state of the structure of the organ. A displacement may even occur to the healthy uterus in consequence of a blow, a fall or a violent effort, and the uterus being kept out of position, altered circulation at once takes place, and tissue change soon follows.

"The position, then, which I assume with reference to the pathological series which may result in confirmed uterine disease, is this: that the pelvic organs of a woman who has hitherto been in perfect health, may become gradually or suddenly diseased by one of the three following abnormal developments in the uterus: 1st, disorder in innervation and circulation; 2nd, change in quantity of connective or muscular tissue;* 3rd, change in position. I assume, furthermore, that the first here mentioned being the primary lesion, the second and third may result from it; that the second being the primary lesion (as in sub-involution or the development of neoplasms), the first and third may result from it; and that the third primarily showing itself in a perfectly healthy organ, the first and second may be its consequences."

It is not necessary that we should dwell further on the merits of this work. The author not only discusses general principles with clearness, bringing to bear on them all the light to be obtained in the most ancient as well as the most modern literature, but he also describes clearly and accurately, in regular order, the several diseases to which the female generative organs are liable, and the best modes of treating them. We commend the work to all who wish to study gynecology on sound and accurate principles.

* In hyperplasia the connective tissue is the part diseased; in sub-involution the muscular tissue is so.

The Principles and Practice of Surgery. By FRANK HASTINGS HAMILTON, A.M., M.D., LL.D. New York. 1872. Pp. 943.

THE name of Frank Hamilton is very well known on this side of the Atlantic, and those who are familiar with his exhaustive treatise on Fractures of Dislocation or that on Military Surgery, will read with much interest a work by him on General Surgery. Nor will that interest be diminished on finding that the author in his preface states the work was intended "to supply, within the compass of a single volume of moderate size, the instructions necessary to a full understanding of all the subjects belonging properly and exclusively to surgery." Whilst acknowledging the real merits of the book and the satisfaction experienced in its perusal, we cannot help thinking that the title, like the author's expressed intention, is a little too ambitious. The man who writes a large volume on fractures and dislocations, and then professes to include the whole of surgery in a book scarcely larger, must either have a rare appreciation of his own powers of condensation, or a profound contempt for every portion of the science and art not included in his former work. Mr. Hamilton's book is an excellent one, so far as it goes, but when we find that Mr. Bryant modestly disclaims for his lately published volume any wish to have it considered more than a "manual," we think that a book which contains one-third less matter can hardly be expected to carry out its author's intentions. The treatise is divided into two principal parts, the first being devoted to general surgery, and occupying more than half the book, and the second part to regional surgery. The first four chapters are given to the description of inflammation and its consequences. In them we find nothing remarkable except the omission of the modern theories and opinions on the origin of pus, such as Cohnheim's, which should have been known to the author. On the subject of gangrene, much importance is accorded to the external use of bromine. This remedy was introduced by Dr. Goldsmith, an American army surgeon, and Dr. Hamilton gives the solution (one drachm of pure bromine in eight ounces of water) unqualified praise in the treatment of unhealthy ulcers, and especially of hospital gangrene. The next few chapters deal with wounds, and although on the subject of repair, the descriptions do not keep pace with the most recent pathology, still the subject is well handled, and the English reader will find much to interest him in

the chapters on gunshot, arrow, and poisoned wounds, and those inflicted by venomous animals. Lesions of the vascular system are disposed of in two chapters, of which one is given altogether to the subject of aneurisms, general and special, with the operations and other treatment for their cure or relief. As we should naturally expect, the part of the book on fractures and dislocations is excellent. In fact, as we find therein expressed, plainly and tersely, the conclusions which had to be elaborately worked out in the author's monograph on the same subject, the section is, for general readers, much more useful. The woodcuts are excellent, most of them being from the larger treatise, but some of them new; and the treatment recommended is perfectly in accordance with that sanctioned by the experience of most practical surgeons. On these subjects the author is so much at home, and is now such an authority, that we can understand his candour when, with a dry humour that forcibly recalls Sir W. Fergusson's style, he confesses his difficulties and failures in the treatment of certain cases. To many a practitioner who only occasionally has a chance of treating a fractured thigh, and finds that, despite all his exertions, shortening results, the following paragraphs, which can be endorsed by every practical surgeon, will no doubt be acceptable:—

“In reference to the prognosis surgeons have not always been agreed; for while a large majority of the most experienced surgeons in all countries, and in all periods of the history of surgery have acknowledged their inability to restore most broken femurs to their original length, a few less distinguished, less observing, or less honest surgeons have from time to time claimed that they seldom or never fail to make these bones unite of the same length as before, if, indeed, they do not sometimes happen to make them a little longer.

“Among those who have not been able to overcome the shortening, except in a few rare cases, are to be mentioned Hippocrates, Avicenna, Celsus, Scultetus, Chelius, John Bell, Benjamin Bell, Velpeau, Nélaton, Erichsen, Valentine Mott, Knight, Buck, Detmold, Warren, and Gross. With precisely the same appliances, or with only such trivial and unessential modifications as could in no way affect the results, a few surgeons, such as South, Hunt, Gamgee, Dorsey, Scott of Montreal, Armstrong, and perhaps some others of no greater claim to recognition as authority, have succeeded always in making perfect limbs.

“I have myself met this latter class of men; and I have also seen the result of their practice, both in the city and elsewhere, and it has never surprised me to find that, so far from always making thighs of the same

length as before, they almost constantly fall considerably short of my own standard of average results. In other words—to state a matter of fact very plainly—through ignorance, inadvertence, or intentionally, these men do not tell the truth.”

Then follow eight valuable conclusions, the result of his own experience.

The chapters on amputation and excision are remarkably well written, though some of the individual descriptions are rather meagre; and after the chapters on disease of bones, joints, the spine, and on club-feet, Part I. concludes with Tumours. Opinions on these morbid structures are now so unsettled that a writer may be excused for taking them in any order, but we think that some broad principle of malignancy or non-malignancy might have been laid down for the guidance of students, and an author who quotes Virchow and Billroth, and professes a great respect for the German school of pathologists, might have been more copious in his extracts, and might certainly have substituted, with advantage, Rindfleisch's description of carcinoma for his own. The regional surgery discusses, in order, the lesions and operations belonging to the several parts of the body. It may be doubtful whether such a special and extensive subject as ophthalmology should be introduced into a general treatise, but perhaps as the work is intended for students as well as surgeons it could not well be omitted. The chapter is plainly written and well illustrated, and may serve as an outline which may afterwards be filled up from some standard monograph. In approaching the end of the volume it would seem as though the author thought he had spent too much time on the surgery of the head, and accordingly makes amends by condensing that of the abdomen and genital organs. Thus the description of hernia is not very full, and the diseases of the urethra, and of the testicles are decidedly too brief. The concluding chapter deals shortly but well, with a subject which owes much to the labours of American surgeons, namely, the surgical diseases of, and operations on, the female genital organs. Although we have pointed out some of the shortcomings of the book, the impression left by reading it is an extremely favourable one. As a student's book it can be strongly recommended, and the English surgeon will probably find much in it that is valuable to him. The style is easy and clear, the print large and good, and the original illustrations, which are numerous, remarkably well done. In expressing this approval we

must, at the same time, say that we have sufficient respect for Mr. Hamilton to know that he could have written a much more complete treatise on surgery, and one that would be more in accordance with the objects he sought.

There is one other point bearing more on the subject of international copy-right than on the merits of the book which, perhaps, should be noticed. Mr. Hamilton reproduces many of the woodcuts from Erichsen, Fergusson, and other English authorities. True, he acknowledges his obligations and the sources, but the propriety of making use of another author's materials to enhance the attractions of a rival treatise may fairly be considered questionable, and may certainly interfere with the good feeling which should exist between men of an honourable profession.

A Treatise on the Diseases of Infancy and Childhood. Second edition, enlarged and thoroughly revised. By J. LEWIS SMITH, M.D., Curator to the Nursery and Child's Hospital, New York; Physician to the Infants' Hospital, Ward's Island; Consulting Physician in the Class of Children's Disease, Out-Door Department of Bellevue Hospital; Clinical Lecturer on Diseases of Children, and Professor in Bellevue Hospital Medical College, New York. Philadelphia: Henry C. Lea. 1872.

DR. SMITH's book is evidently the result of practical work, and as such it is with great pleasure we hail its arrival at a second edition.

It is well got up, and arranged in a thoroughly practical manner, having a well-devised table of contents, and an accurate index at the back. The print is clear and the type large, and there are some very good drawings interspersed through the work.

It is divided into three parts. The first part treats of infancy and childhood, care of the mother in pregnancy, mortality of early life, &c., lactation, selection of a wet-nurse, weaning, artificial feeding, baths, clothing, &c.; accidents to the infant at birth, conjunctivitis, diseases of the umbilicus, and the diagnosis of infantile diseases. The second part is divided into three sections, the first section treating of constitutional and diathetic diseases, such as

rachitis, scrofula, tuberculosis, syphilis, &c.; the second of the eruptive fevers; the third of the non-eruptive contagious diseases, diphtheria, pertussis, parotiditis; &c.; while the fourth section enters fully into general diseases, such as intermittent, remittent, and typhoid fever, acute rheumatism, erysipelas, &c.

The third part is divided into five sections; the first section consisting of sixteen chapters, enters fully into all the diseases of the cerebro-spinal system, and we at once say that a perusal of this chapter alone will amply repay the reader. The second section, on diseases of the respiratory system, although short, is practical and well written. The third section, which is devoted to the diseases of the digestive apparatus, is certainly pregnant with information, all of a practical nature, and derived from the author's personal experience. The fourth section is very short, treating merely upon the literature, etiology, and treatment of cyanosis. The fifth section is devoted entirely to diseases of the skin and its appendage. In addition to all this there is a most valuable appendix added, consisting of—A. A dietary formula, which is full of plain, useful and practical information upon the proper foods for infants, a matter which, we think, requires some ventilation. B. Remarks on the prevention of scarlet fever by William Budd, M.D. C. Remarks on the diphtheritic membranes by Dr. Edward Rindfleisch. E. Observations on the state of the liver in infantile enterocolitis; and F. Cases of intussusception, &c., &c.

From the space at our disposal it would be impossible fully to review this valuable work. We will, however, give some extracts, which we consider of especial interest.

In the article on dentition the author speaks plainly upon the advisability of scarifying the gums. In his own words—

“The gum lancet is now much less frequently employed than formerly. It is used more by the ignorant practitioner, who is deficient in the ability to diagnosticate obscure diseases than by one of intelligence, who can discern more clearly the true pathological state. Its use is more frequent in some countries, as England, under the teaching of great names, than in others, as France, where the highest authorities, as Rilliet and Barthez, discountenance it.

“It is well to bear in mind, as aiding in the elucidation of this subject, the remark made by Trousseau, that the tooth is not released by lancing the gum over the advancing crown. The gum is not rendered tense by pressure of the tooth, as many seem to think, for, if so, the incision would not remain linear, and the edges of the wound would not unite, as they

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"There exists an opinion in the profession that the liver is in fault in this disease, especially in that form of it which I have described as a summer epidemic of the cities. This opinion is, probably, less prevalent than formerly, but it is still held by many, and it influences more or less the choice of therapeutic agents. In the appendix (E) is a table, which presents the condition of the liver in thirty-two cases of this disease. These cases occurred during the summer epidemic.

"There was no evidence from the *post-mortem* appearance of the liver in these cases, of any congestion or torpidity, or hyper-activity, or perverted secretion. The size of the liver was in some cases very different in those of about the same age, but probably there was no greater difference than usually obtains among glandular organs within the limits of health. The following table gives the weight of the liver in twenty cases in which the weight of this organ and the age of the patient are recorded:—

Age.		Age.	
4 weeks	5 ounces.	10 months	6½ ounces.
2 months	3½ "	13 "	6 "
2 "	3½ "	14 "	9 "
4 "	5 "	15 "	6 "
5 "	6½ "	15 "	7½ "
5 "	9 "	15 "	9½ "
7 "	4½ "	16 "	6 "
7 "	6 "	19 "	4½ "
7 "	6½ "	20 "	9½ "
9 "	8 "	28 "	15 "

"I do not have access to tables giving the weight of the healthy liver at different ages, but in none of the above cases did the size or the weight seem to me to be above the healthy standard, except in one, in which this organ was quite fatty. But in this case the degeneration and enlargement of the liver were doubtless due to the tubercular disease.

"In most of the cases the liver was examined microscopically, and the only fact worthy of note observed was the variable amount of fatty matter. Sometimes it was in excess, sometimes in moderate quantity or rather deficient, and sometimes in greater amount in one portion of the organ than in another.

"The prevalent belief, then, that the liver is greatly affected in the summer epidemic of entero-colitis, receives no corroboration from the inspection of this organ. The only pathological state (if it be such) observed in it relates to the amount of oily matter, and this obviously requires no special treatment."

Again, the author says, speaking of the troublesome cough, which is so frequently present in these cases:—

"It has been said, in speaking of the symptoms, that a cough is common

in the advanced stage of entero-colitis, particularly when the disease is protracted for weeks or months. From the great emaciation and the character of the cough, the physician as well as friends is very apt to suspect the presence of tubercles. In the eighty-two examinations, however, which I have made of entero-colitis of the summer season, in many of which emaciation was extreme, there were tubercles in only one case. The cough was found to be due to solidification of the posterior and dependent portion of one or both lungs. The exact pathological character of this solidification of lung (hypostatic pneumonitis) is treated of in our remarks on diseases of the respiratory organs.

"In the case of entero-colitis which were complicated with this state of the lungs, I have not usually found enough of the lung tissue involved to make any perceptible difference in the sound on percussion. Its extent of solidification was sometimes not more than two or three lines, and frequently not more than a quarter to half an inch in an antero-posterior direction, although it embraced nearly or quite the entire posterior surface of the lung."

Of the condition of brain he says:—

"The state of the brain in the entero-colitis of infancy is interesting to the pathologist. When the disease is protracted, this organ wastes like the body and limbs. In the young infant, in whom the cranial bones are still ununited, the occipital and sometimes the frontal become depressed in proportion to the loss of brain substance, so that the cranium is quite uneven. In older children, with the cranial bones consolidated, serous effusion occurs, according to the degree of waste, thus preserving the size of the encephalon. The effusion is chiefly external to the brain, extending on each side over the convolutions from the base to the vertex. The quantity of serum varies from one or two drachms to an ounce, or even more. The serous effusion is associated with passive congestion of the cerebral vessels and cranial sinuses."

We regret that our space has not permitted us to enter more fully into the merits of this work, which we heartily commend to our readers as a practical and trustworthy guide to the diseases of infancy and childhood; it is in every respect what the author represents it to be, viz., succinct, yet comprehensive, and containing all recently ascertained facts relating to this branch of medical science, while the author has respected the opinions of previous writers, and adopted them so far as appears to him correct. He has depended, he states, more for the material of his treatise on clinical observation and the inspection of the cadaver. Necessarily, as a result of independent investigations, opinions are now and then

expressed differing from those which are commonly accepted. Novel views, however, have not been presented without a sufficient number of observations to substantiate them. This (the second) edition is much enlarged, nearly twenty additional diseases being treated of, and although the author states he has made every effort to prevent an undue enlargement of his book, yet from the many recent discoveries in pathological histology, it is necessarily enlarged by nearly one hundred pages.

Traité Pratique des Maladies du Larynx et du Pharynx. Par le DOCTEUR L. MANDL. Paris: J. B. Baillière et Fils. 1872. Pp. 816.

IN the July number of the Journal we noticed the third edition of Dr. Morell Mackenzie's work "On the Use of the Laryngoscope," and his "Essay on Growths in the Larynx," both of which we were enabled to speak of with the commendation they deserve. The large volume at present under review, although further showing the extended field for observation which has been opened by the application of the laryngoscopic mirror, is, however, of quite a different scope from those excellent and practical monographs; it is, in fact, a systematic treatise on the diseases of the larynx and pharynx, as well as a complete anatomical description of those parts, their physiological functions, pathological anatomy, and therapeutics. The author, Dr. Mandl, has for a number of years been well known at the *École Pratique* of Paris for his researches in histology and histogenesis, and as a writer on the physiology and therapeutics of diseases of the respiratory tracts; as a further result of his labours in the latter direction he has now presented us with a book, "the *résumé*," he tells us, of the experience "of more than twenty years devoted to practice and instruction in pharyno-laryngeal affections."

The work is divided into five parts: in the first the anatomy of the larynx and pharynx is minutely described; in the second, after a brief introduction on the principles of optics as applied to the laryngoscope, a description of that instrument is given, and the different modes of its illumination and various details connected with its employment, as well as the difficulties to be overcome in the examination and demonstration of the larynx and posterior nares, are fully explained. The physiology of the larynx and pharynx is treated of in the third part, and embraces a large and elaborate

portion of the volume. In it are studied the principles of acoustics and their application to various forms of musical instruments; the subject of phonation, including the mechanism of the production of sound, the characters which it presents, and the physical conditions of the vocal instruments, etc.; the vital properties (sensibility and mobility) of the larynx and pharynx; and the rôle which the latter also fill in the functions of respiration and digestion. General pathology, including etiology and general therapeutics, comprising a detailed description of the different modes of topical treatment, etc., occupy the fourth part, and in the fifth special pathology and treatment is discussed. The text is intercalated with numerous wood-cuts, in addition to several beautifully executed plates of lithographic engravings, both plain and coloured; there is also a copious bibliography. The plan of the work is one which, logical enough although it may be, seems to us to give rise to considerable prolixity, unavoidable repetition, and frequent back reference, which, in a book without an index, like most continental publications, leads to trouble and loss of time. Each part, moreover, is divided into sections, chapters, articles, and numbered paragraphs, besides introductions and appendices—an arrangement rather foreign to an English reader, and necessarily very restrictive in style. The subject-matter, however, is excellent, evidently based on long observation, and will well repay by its perusal those who are interested in the study of a still comparatively obscure class of diseases. The execution of the letter-press and illustrations is admirable, and the entire work produced in a manner worthy of the reputation of its well-known publishers.

In consequence of our former notice of Dr. Mackenzie's works it is unnecessary here to do more than mention that Dr. Mandl describes and figures most of the instruments and appliances which have been or are at present in use in the treatment of laryngeal diseases. Of these Dr. Mandl has himself invented several, none of which, however, seem to possess any advantage over the less complicated ones Dr. Mackenzie employs.

The descriptions of the various dispositions of the larynx and of the mechanism by which they are accomplished in the phenomena of normal and modified respiration, and in the production of sound in its different modifications and registers are clearly given and excellently illustrated. The differences of opinion that formerly existed among physiologists as to whether the larynx should be considered as a wind or string instrument have, since the application

of the laryngoscope, been settled in favour of the latter; but there are still doubts as to the form of the orifice of the glottis; in the different registers and modifications of the voice most authors believe that there is a progressive occlusion of the orifice of the glottis with elevation of sound. Dr. Mandl, however, affirms that it vibrates in its full extent during the emission of sounds of the inferior register (chest voice), and in its upper or interligamentous portion alone in those of the superior register (falsetto or head voice), the intercartilaginous portion of the orifice remaining firmly closed.

In *primary* tubercular laryngitis the first change, according to Dr. Mandl, that is observed by the laryngoscope, is "the presence on the anterior surface of the posterior wall of the larynx of *primordial vegetations* more or less numerous, of different sizes, of the colour of the adjoining mucous membrane or paler, even whitish, and having the greatest analogy to the syphilitic vegetations on a cock's comb." These "polypoid productions" may, as in a case he relates, occur in an otherwise apparently robust and healthy individual, in whom the most minute examination cannot determine the existence of tubercles elsewhere or of any syphilitic affection, and who may only complain of hoarseness.

Laryngeal phthisis, *secondary* to tuberculosis elsewhere, on the contrary, he distinguishes as commencing by a superficial inflammation of the mucous membrane—

"This superficial inflammation appears also in the primitive affection a short time after the vegetations; then the anatomical alterations which supervene are identical in the two forms." "Hoarseness is due more frequently to swelling than to ulceration of the vocal cords; the tumefaction of the arytanoids and ventricular bands (false vocal cords) which cover the vocal cords, rendering the vibration of the latter impossible."

Consecutive laryngeal phthisis, according to the author, is more common in men than women in the proportion of three to one, and declares itself more frequently when the right lung is the principal seat of the tubercle. This difference he seeks to explain by the compression that is exercised by the tubercles, situated in the summit of the lung, on the recurrent nerve, which runs on the left side between the trachea and œsophagus, and thus escapes, protected by the latter, from the compression which leads to fatty degeneration of the corresponding intrinsic muscles. Dr. Mandl notes as an important clinical fact, that—

"An essential nervous aphonia—viz., bilateral dynamic paralysis of the tensors of the vocal cords (crico thyroideans)—may perhaps be in young girls the precursor of a tubercular inflammation which declares itself later. The return of the voice by the application of electricity is not an absolute security. It is in such cases especially that it is necessary to abstain, according to Trousseau, from the employment of ferrugineous preparations, which determine a sanguineous plethora by no means devoid of serious inconveniences in individuals predisposed to hæmoptysis and to tuberculization."

For a long time Dr. Mandl also has proscribed the use of iron in chronic laryngitis and pharyngitis, as the plethora consecutive to its administration turns into a local hyperæmia; consequently, chronic phlegmasias are more often kept up by it than amended.

That particularly troublesome complaint known as granular (follicular) pharyngitis or clergymen's sore-throat—generally, so chronic in its nature, and, though often temporarily relieved, so apt to relapse—Dr. Mandl has succeeded in curing, by painting the granulations twice a day, with a solution composed of one part of metallic iodine and one of carbolic acid, dissolved by means of iodide of potassium, in one hundred parts of glycerine. If irritation supervenes the application is less frequently applied or superseded for a time. The largest granulations are first scarified, and then touched with the glycerole, but in a more concentrated form, and in variable proportions, according to the degree of the affection. This local treatment alone is, he believes, sufficient to radically cure the disease independently of any supposed diathesis. Dr. Mandl may probably have been led to adopt this mode of treatment from Dr. Hastings, who (in his book on "Diseases of the Larynx and Pharynx," London, 1850, and enumerated in Dr. Mandl's bibliography) recommended the application to the "mucous crypts which had previously resisted the remedial effects of nitrate of silver," of a "saturated solution of iodine in rectified spirit."

Posological Tables; being a Classification of the Doses of all Official Substances, for the use of Students and Practitioners.
By W. HANDSEL GRIFFITHS, Ph. D., L.R.C.P., L.R.C.S.E.;
Assistant Librarian to the Royal College of Surgeons in Ireland.
Dublin: Fannin & Co.

IN these tables Dr. Griffiths has arranged the various substances used in medicine, and the various pharmacopœial preparations in

groups, so that their respective doses may be seen at a glance. The chart on which the tables are printed is intended to be placed upon the wall for ready reference; it is of a convenient size, and is admirably adapted for the resident pupils' rooms of hospitals, for dispensaries, for compounding establishments, and for the rooms of students, who will find the tables so arranged on the chart as to bring the memory of the eye to aid them in recollecting the preparation and doses of medicines.

On the Treatment of Diseases of the Skin, with an Analysis of 11,000 consecutive Cases. By Dr. M'CALL ANDERSON. London: Macmillan & Co. 1872. Pp. 180.

THE Glasgow Dispensary for Skin Diseases is, we believe, the chief, if not the only, nursery of dermatology north of the Tweed, and Dr. Anderson has already proved to what account he has turned the opportunities there afforded by his three monographs on Psoriasis, Eczema, and the Parasitic Affections of the Skin. Last year he published in the *Lancet* a series of papers, which are now reprinted, and brought out in a handy octavo volume. Through an accidental oversight Dr. Anderson's book escaped being noticed in our late review of works on skin diseases, and we gladly hasten now to repair the omission. The work embraces two distinct parts, the first of which is occupied with an analysis of 10,000 cases of skin disease met with in hospital practice, and of 1,000 in private practice; and the second with a general survey of the therapeutics of these affections. In Part I. a curious case of the so-called bloody sweat, *epidrosis cruenta*, is mentioned, which the writer had previously described at length in the *Journal of Cutaneous Medicine*, and which apparently was an example of vicarious menstruation.

The most noteworthy feature in the statistics is the remarkable frequency of parasitic diseases, for, out of the 11,000 cases, no less than 3,561 were dependent upon the presence of parasites. Of these, the vegetable parasitic affections numbered 656, including 160 cases of *favus* (only four of which occurred in private, all in members of the same family), an extraordinary frequency of that disease when compared with its rarity in this country. In Dublin the diseases due to fungi seem to form scarcely 3 per cent. of the total number of cases occurring among the poorer classes, and, even

at special dispensaries, not more than two or three cases of favus will crop up in the year.

Tinea decalvans figures in 197 cases, forty-four of which were observed among the 1,000 private cases, and the author distinctly maintains the parasitic nature of this disputed affection. The reason, according to Dr. Anderson, that the fungus is so generally overlooked is, that it is very small compared with the other vegetable parasites, and that it implicates the sheath of the hair rather than the hair itself, so that when the latter is extracted without the sheath, as so often happens, no fungus is discovered. But he does not deny the probable existence of cases of local alopecia, due to faulty innervation. The affections due to the presence of animal parasites were 2,905 in number, viz., 2,571 cases of scabies, and 334 of phthiriasis. Here, again, scabies seems to be very much more plentiful in Scotland than it is with us, or in London, according to the statistics of Dr. T. Fox. Two cases of that peculiar form of purpura known as *peliosis rheumatica*, are given, a disease of which we have seen but one example.

The therapeutical part is treated in a suggestive rather than in an exhaustive manner, and forms a good practical digest of the most important lines of treatment. Speaking of the popular dread of "driving in" an eruption, a fear shared by some in the profession, the author justly remarks that those who have most experience in the treatment of diseases of the skin will probably agree with him in thinking that the difficulty lies, not in curing them sufficiently slowly, but in curing them with sufficient rapidity.

Sundry useful hints may be gleaned here and there from the pages of this book. For example, we learn that the most soothing form of poultice is that made with cold water and potato starch; storax ointment is preferred in the management of scabies; and Ricord's treatment for syphilitic condylomata is pronounced to be very successful. This consists in washing the parts twice a day with solution of chlorate of sodium, drying them, dusting with calomel, and separating them, say at the anus, with a piece of clean dry lint. We were tempted to quote a formula for a depilatory which the author finds to be efficient without being too irritating, but we will refer our readers to the book itself, and can assure them that they may derive much profit from a perusal of the fruits of Dr. Anderson's wide experience.

The Causation of Sleep: a Physiological Essay. By JAMES CAPPIE, M.D. Edinburgh: 1872.

THIS is an ingenious and pleasant little book on a subject which is still very obscure. We shall try to give very briefly a sketch of the author's theory, referring all those who are interested in the physiology of sleep to the work itself for further details. Dr. Cappie is one of those who believe that the quantity of blood in the cranium cannot undergo very rapid change, and that if the arteries become small the veins must undergo a proportionate increase in size. He starts with the assumption that sleep and coma differ only in degree, and as coma is frequently due to increased pressure, so sleep may be due to the same cause. The pressure occurring during sleep is exerted by the distended veins on the surface of the brain. The blood in the vessels of the brain is subject to two forces, the force of the heart exerted on the blood in the arteries, and the pressure of the atmosphere on the veins in the neck, tending to keep the blood in the skull. While a person is awake the attraction of the brain tissue for blood is so great that the arteries are kept full, and the veins, which lie chiefly at the surface, are comparatively empty. But when the brain becomes weary or exhausted, its attraction for arterial blood diminishes, the arteries contract, and, to fill up the void in the cranium, and, yielding to the pressure of the air, the veins become distended. These distended veins press on the grey substance of the convolutions, and the physiological result of this pressure is sleep.

In this theory we think the author places too much weight on the attraction of the tissues for blood, or, as it is generally called, the "capillary force," as one of the forces which carry on the circulation. The existence of this force has never been proved, and since the discovery of the rhythmical and peristaltic contractions of the arteries, it is quite unnecessary to account for the phenomena of the movements of the blood. Again, we are quite ready to admit that when the cerebral arteries contract the veins dilate, but we cannot understand how it is that the pressure on the brain can be increased in this way. The force of the atmospheric pressure must act on the veins, whether the arteries be dilated or contracted, and the effect on the brain of the closure of the arteries must be only to diminish the force exerted on it by the heart. We think that sleep is mainly due to the deficient supply of arterial blood to the brain, and this is caused by the contraction of the small arteries. How this

contraction is brought about, whether by some change in the brain itself or in the vaso-motor nerve centres of the medulla oblongata, or cord, is the still unsolved part of the problem. The author seems to us to have confounded cause and effect in his theory. Pressure produces coma by expelling the blood from the arteries of the grey substance, but the venous distension, which probably occurs during sleep, is secondary to the emptying of the arteries by contraction of their own coats, and we fail to perceive any mechanism by which this distension can rise to such a degree as to make the intracranial pressure greater than during the waking state. The whole subject, however, is one of the greatest possible obscurity, and we cordially welcome Dr. Cappie's work as an attempt towards the solution.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Wednesday, March 18th, 1873.

DR. FITZPATRICK, in the Chair.

Remarks on the Semeiology of Chronic Alcoholism. By ALFRED H.
M'CLINTOCK, M.D.

IN bringing under the notice of the Medical Society the great subject of alcoholism, I wish at the outset to dispel any idea that I am about to submit an elaborate compilation or original essay. To do either the one or the other is quite out of my power. All I aim at is to look at it from one point of view—to study one of its aspects which, I think, possesses a high degree of practical importance.

It is matter of notoriety that the immoderate use of diffusible stimulants has greatly increased of late years among classes whose education and social condition should have been sufficient preservatives against this destructive vice. It is well known to physicians that alcoholism is to be met with among the occupants of drawing-rooms as well as the frequenters of tap-rooms. What has so widely spread this pernicious habit among the present generation, it would be difficult to say. Probably more causes than one have contributed to bring it about. I cannot help thinking that the intense strain put on the intellectual faculties in every profession, trade, and pursuit, the mental competition of these days, must create or foster an appetite for diffusible stimulants. Nervous exhaustion craves for the alcoholic stimulus; whereas physical or muscular exhaustion will more eagerly demand solid aliment. Furthermore, candid, I must own to a feeling that our own profession is not free of blame in this matter. I know well I am here trenching on delicate ground, but, "*Amicus Plato, amicus Socrates, magis amica*"

and I strongly suspect that the modern treatment of disease by the free use of stimulants, though fulfilling an important therapeutic indication, has occasionally developed a morbid habit or taste, eventuating in moral and physical disease of an incurable kind. I would qualify this, however, by adding that the declaration of inebriates themselves on this point is of no value whatever, as they are notoriously untruthful, and are always but too glad to throw the blame of their evil habit on the physician or surgeon.

If there be any foundation for the above suspicion—and nothing would give me more satisfaction than that it should be disproved—it certainly increases the responsibility resting on us to discover this pernicious habit whenever we can, and to use our utmost endeavours to eradicate it.

But, even in the absence of any moral considerations of this kind, a regard for our professional character and for the credit of our art should stimulate us to acquire a knowledge of all the symptoms which may attend, in varying degrees, on the habit in question, so as to be able to distinguish them from the effects of idiopathic disease, and to trace them to their true source.

The term alcoholism was first used by Magnus Huss, of Stockholm, about the year 1852, and has been adopted by succeeding writers, French and English; and perhaps no better term could be employed to express the pathological effects of over-indulgence in the use of diffusible stimulants. He describes two forms of alcoholism, viz., the acute and the chronic. Of the former (*i.e.*, acute alcoholism) we have the best and highest example in delirium tremens; but every case of drunkenness should fall, I think, under the same head, and, properly speaking, is an instance of acute alcoholism.

Chronic alcoholism, according to the sense in which Huss, Marcet, and other writers use the term, expresses a train of symptoms belonging to the nervous system, which symptoms may continue or be developed long after the pernicious habit has been relinquished. Now, I think it will be freely admitted that a large class of cases are met with of deranged *organic* functions, which derangements are the direct result of the abuse of spirits in some form or other.

These cases constitute a numerous category, and it is chiefly of their symptoms I attempt to give a brief description.

The task, then, I have proposed to myself in this paper is comparatively a very narrow one, briefly to pass in review the effects which are produced on the bodily functions by intemperance; in other words, the symptoms and disturbances which it gives rise to, and by which the baneful vice may be suspected or discovered. To form a correct opinion on this point is assuredly of much importance, as failing in this we may commit serious errors of diagnosis and prognosis, and expose ourselves and our art to

ridicule and contempt. In every instance coming before us we need to see well that the grounds of our diagnosis are sure, before breathing a suspicion that would tend to criminate the patient; and even when fully confident in the correctness of our opinion, the utmost tact, delicacy and circumspection are often requisite in the mode of communicating it to patients or their friends. It must be remembered that all these patients, with scarcely one exception, conceal how much they drink over and above the limit of moderation, and will deny in the most emphatic and persistent manner that the quantity is at all inordinate. Herein lies the source of our greatest difficulty in dealing with them. The utter disregard of truth evinced by most of them when questioned about their ruling vice, is one of the most striking and melancholy features of moral depravity which they present. I have seen a lady of education and refinement, and the mother of a large family, protest most solemnly that there was not the slightest foundation for my suspicion, and even when confronted with the half finished bottle of brandy, brought forth from its hiding place, and by the messenger she had sent to purchase it, still with unblushing assurance did she resist the direct and circumstantial evidence which proved her a liar of the deepest dye!

To my mind the demoralizing, debasing effects of intemperance can not be more strongly shown than by this extinction of the principle of truthfulness—a principle which lies at the foundation of all morality. On this point Dr. Skae in his essay on Dipsomania (*Winslow's Journal*, Vol. xi.) thus graphically expresses himself:—

“Such persons are singularly mendacious. They will resort to every possible device to procure stimulants, to excuse their conduct, to deceive their friends and medical attendant, and will display an ingenuity and fertility in deceit which is truly marvellous.” . . . “They will evade the most vigilant surveillance, and tell the most deliberate falsehoods in their attempts to deceive, solemnly appealing to God for their truth. When shut off from the ordinary sources of stimulation they will sometimes resort to almost anything in order to relieve their craving. I have known a young and delicate lady after being prevented getting wine or spirits, and deprived of red lavender, lavender water, and eau de Cologne, take creosote, vinegar, vitriol, and tobacco.”

In an inquiry like the present we need not stop to consider whether, or how far, alcoholism is the result of a vice or of a disease, though I admit the importance of such a distinction in a medico-legal point of view, as well as in legislating for inebriates.

With regard to the effects of spirituous liquors on the health and constitution, we might divide all drinkers into three great classes:—

1st. Those who partake of them in moderation, and who are benefited by their use. Thus, Dr. Thomas King Chambers, in summing up the results of some carefully conducted experiments, speaks on this wise:—

"In short, the circumstances seem to indicate that a moderate dose of alcohol acts by temporarily augmenting the digestive power of the stomach, helping it to appropriate more thoroughly the food."

The 2nd class are those who partake of alcoholic liquors in larger quantity and for a lengthened period, and who, consequently, may be said to be intemperate, although they do not, perhaps, at any one time, drink to such excess as to lay themselves open to the charge of being "drunk and incapable," or "drunk and disorderly," or, indeed, of being drunk at all, in the true sense of the word. They are guilty of excess by the total quantity consumed in the twenty-four hours, and not by that taken on any one occasion. To this class of drinkers, I believe, belong a large proportion of the cases of chronic alcoholism, met with in the upper ranks of society, and especially among females of this rank.

Furthermore it would seem demonstrable that the intemperance of the upper classes in the present day consists very much in the oft-repeated ingestion of some kind of alcoholic stimulants—whether it be wine, spirits, or the favourite B. S. (Brandy and Soda). In this respect the practice of our contemporaries differs much from that of our forefathers, who confined their potations to after dinner, and, as a rule, took little or nothing before that time. Which system inflicts more injury on the health is a question that might admit of considerable discussion, and though it does not properly fall within the scope of this paper, I may just remark, *en passant*, that the testimony of Dr. King Chambers, who has been at some pains to investigate the actions of alcohol upon the economy, goes far to show that small and frequently repeated stimulation is much the more injurious mode of taking alcohol. "It is very clear," he observes, "that alcohol, taken in the dram drinkers' fashion, in small divided doses, by no means increases metamorphosis. It rather tends to diminish it, and this diminution is not sudden or immediate, but is more and more, for a certain period, till the retention reaches a point at which a critical discharge takes place in healthy persons." And, again, when laying down rules for the use of alcohol in diseases, he enjoins, "Divide the daily allowance into two or three doses only, giving enough at once to produce a decided effect. The action of frequent small divided drams is to produce the greatest amount of harm of which the alcohol is capable, combined with the least amount of good."

Of the evil effects of drinking in the early part of the day he uses the following decided language:—"I have not yet met with a forenoon tippler, even though he never got drunk in his life, without a condition of stomach which most infallibly shortened his days."

The 3rd class of drinkers comprehends those whose allowance of liquor is so immoderate as to produce full intoxication. These are the out and out drunkards, who drink without shame or compunction, and who know not any limit to their debauch but the length of their purses or

the capacity of their stomachs. In such cases as these there is generally much less difficulty in coming at a knowledge of the habits of the individual than in persons of the second class, whose rule of drinking seems to be "a little and often."

Nearly all the cases of alcoholism which have fallen under my immediate observation were females of the better class in society. All, with scarcely an exception, concealed the habit they had acquired of drinking to excess, so that it often needed a lengthened investigation and a careful analysis of all the symptoms, before any positive conclusion, as to the real cause of the patient's condition, could be arrived at.

All sought medical advice on account of some derangement belonging to the alcoholism, though apparently not themselves recognizing the source from whence their ailments sprang. I should qualify this by saying that a few were brought by friends who had no suspicion of the patient's habits. The intoxicating liquors taken, varied a good deal, viz., brandy, gin, champagne, port, sherry, whiskey, ale, porter, and, in one case, tincture of ginger—of which her daily allowance, for a length of time, was one pint. This unfortunate lady, who moved in a high circle, and was what is ordinarily called a strong-minded person, eventually died of jaundice, enlarged liver, and ascites. It is very well known that where the ordinary spirits cannot be obtained, the victims of this passion will not hesitate to drink eau de Cologne, lavender-water, sal volatile, compound spirits of lavender, and, in fact, any spirit they can lay their hands on. Most of us here have known, or heard of the museum porter at one of our schools of medicine, who used to drink the spirits of wine off the anatomical preparations, when he had not the means of buying a more potable description of alcohol. In justice to his palate I must add that this was before the methylated spirits came into use.

The ingenuity displayed by many of these individuals to hide the vice, and elude the precautions for keeping away the much coveted drink, is sometimes most curious. For example, a lady with this propensity very strong, was strictly confined to one room by her family, who allowed nothing to enter without its being carefully searched for the contraband article; nevertheless in spite of all these precautions she managed to have spirits conveyed to her, not, indeed, in a bottle, jar, or jug, but in the undisguised and unsuspected *pot de chambre*!

Of my patients the youngest was a girl of nineteen, whose mother died from the effects of intemperance. Nearly half the number were widows. Four cases have come under my immediate observation where death took place in consequence of organic diseases induced apparently by the long continued use of intoxicating liquors. One of these cases I have already alluded to, as dying of jaundice, anasarca, ascites, and enlarged liver. Another, with fatty enlargement of the liver, died of exhaustion from long continued vomiting; and two died of cirrhosis of the liver and dropsy.

According to Dr. Marcet, "women appear to be much less subject to suffer from the long continued abuse of alcoholic liquors than men;" "but," says Dr. Forbes Winslow, "if the effect of the vicious habit be measured by the mortality, it will be seen from Mr. Neison's data that the reverse is probably the truth."

I now come to speak of the *symptoms*. And here I cannot help remarking that many writers, of the highest authority on this subject—for example, Huss, Winslow, Anstie, Skae, Marcet, Leon, Thomeuf, &c., have confined their observations to the effects of alcohol on the nervous system, and give us little or no information about its effects on the liver, stomach, kidneys, intestinal canal, skin, and other parts subservient to *organic* life. Chiefly to this latter group, therefore, I purpose confining my observations, as I could add nothing to what the foregoing and many other authors have written in regard to the effects of acute and chronic alcoholism on the functions of *animal* life.

1. With persons given to excess in the use of alcohol, there is a peculiar odour of the breath so characteristic that, from it alone, one may almost predict the habits of the individual. It possesses somewhat of an ethereal taint, and has, at times, a strong resemblance to the odour given off in saccharine fermentation. Once perceived it is not readily forgotten or mistaken. Its presence always admits of detection, and, when detected, it suggests the direction our inquiries ought to take. Guided by this symptom, I have many a time been saved from committing blunders of diagnosis and treatment. It has awakened the first suspicion, and thus supplied the first clue to the real cause of the patient's ailments. These patients are generally themselves conscious of their breath being impure, and they will artfully try to prevent your getting a full whiff of it; hence, an averted mouth, or the hand kept on it when speaking, may sometimes enable one to suspect the patient's habits, even before catching the odour of the breath. In reference to this point, Dr. Anstie writes, "One symptom it is not easy to explain, but which nearly always exists, even where there are no signs of dyspepsia, is a peculiar foul breath smell, which it is impossible to describe, or to mistake when once it has been smelt. It is quite unlike the odour of the alcoholic liquor itself, and may be separately distinguished, even where the latter is also present."

The experiments of Lallemand, Perrin, Duroz, and others, seems to show that alcohol undergoes no chemical change in its passage through the body, and that none of it remains behind. Now, the character of the breath I have described, is only observable for a comparatively limited time—some hours probably—after the ingestion of alcohol, and is, perhaps, due to its presence in the expired air. This might go far to account for the cough which, according to Dr. Craigie, is an invariable symptom with all spirit drinkers.

2. Vomiting, as every one knows, is a common effect of a drunken

debauch. But vomiting may also present itself in a more persistent way in consequence of the immoderate, though not intoxicating, use of stimulants. The irritability of the stomach is often such that for many hours the organ will not tolerate the smallest quantity of the blandest fluid. Some of the worst cases I have ever met with of persistent convulsive vomiting were of this kind. In one instance—that of a young married woman—the protracted violent efforts of vomiting and retching were most painful to witness, and well nigh threatened to extinguish life by their uncontrollable severity and long continuance. This patient used to take about a flask of brandy, with a free allowance of champagne in the twenty-four hours, but yet never presented the appearances of intoxication. She died of exhaustion in one of these attacks, and the necropsy revealed *no disease of the stomach beyond some slight congestion*, but the liver was somewhat fatty. Dr. Chambers relates the case of a lady dying under precisely similar circumstances. The symptoms “were those of simply retching and sinking, and the nature of the case was made apparent by her desiring her maid to bring her a glass of brandy, even while I was speaking to her.”—(Case XCI).

This alcoholic vomiting may, to a certain extent, admit of being recognized, by its coming on in the morning and forenoon—by its extreme severity—and by its independence of any organic lesion, or other discoverable cause, such as pregnancy or Bright’s disease. At the same time we must take into our consideration all the accompanying symptoms and circumstances of the patient, that we may see how far these confirm or negative the above conclusion.

The morning sickness, in Dr. Anstie’s opinion, “is not a mere dyspeptic disorder, but a true part of the nervous phenomena of a receding narcosis.”

Along with this symptom there often exists, as Dr. Budd has remarked, an inflammatory or catarrhal condition of the gastric mucous membrane, which may give rise to flatulence, loss of appetite, and other disorders of digestion so common in persons addicted to intemperance.

3. There is undoubtedly a form of Diarrhœa occasionally induced by acute chronic alcoholism, and long before any structural disease has taken place in the stomach or liver. After any excess in the use of alcoholic beverages many persons get an attack of diarrhœa, and this from its cause has received the epithet of *crapulous diarrhœa*. It would seem to be excited in consequence of the pylorus allowing the food to pass in an undigested state, so that it acts as an irritant to the intestinal canal, causing increased exhalation and peristaltic action, accompanied by pain and griping. Examination of the fæces shows the imperfectly digested state of the food, which is commonly acid, and sometimes putrid, from an insufficient admixture of bile to prevent decomposition. Besides this *crapulous* form, diarrhœa is sometimes met with in other cases of chronic alcoholism, even where the intemperance is of a moderate kind, never

amounting to intoxication. Here I cannot attach to it any peculiar or pathognomic characters. It would seem to be the consequence of a congested and irritable condition of the intestinal mucous membrane, so that very trifling causes are sufficient to provoke increased action of the bowels; and this increased action is generally more troublesome early in the morning, or in the forenoon. I have seen this kind of diarrhoea kept up for months by the habit of taking a "night cap" of brandy or whiskey at bedtime; and to be permanently cured by giving up this hurtful practice.

4. Nothing is more common in persons of intemperate habits of every degree, than a slight tinge of jaundice, seldom amounting to more than what would be called a bilious hue in the conjunctivæ. This sometimes succeeds to vomiting (and may occur no matter what has caused the vomiting); and it may result from organic disease of the liver, induced by the prolonged use of alcoholic drinks; but, independently of these classes of cases, we meet with it as a symptom of chronic alcoholism. The non-elimination of this bile, whose retention in the blood causes the jaundice, is, most probably, due to the influence of alcohol in retarding or diminishing vital metamorphosis. This icterode tinge of the sclerotic is well known to be a very constant and characteristic feature of the habitual drunkard's physiognomy; but even in the milder cases of intemperance it is occasionally present, and thus serves to aid our diagnosis.

5. I am disposed to think that among the class of moderate drinkers, the appearance of acne in any of its forms is rather exceptional; still the occurrence of acne on the forehead or nose—I do not so much suspect it on the chin—should awaken a suspicion that all is not right, and urge on further inquiries in the alcoholic direction. I have seen marked examples of acne indurata and acne rosacea, induced by the habit of taking spirits at bedtime; which entirely subsided when this habit was given up. This recalls to my mind the case of a young lady I attended for some time on account of indigestion, and frequently recurring acne on the lower part of the face. She seemed to derive little benefit from all the medication and careful regulation of her diet. In truth, her age, her social rank, and refined manners, lulled every suspicion that crossed my mind. At length, she accidentally dropped the observation that no anodyne was so effectual as gin, in relieving her nocturnal wakefulness and fidgettiness—symptoms, I may here remark, which are very often complained of in all cases of alcoholism. This admission led me to cross-examine her a little more closely, when I made out that she was accustomed to take a wine-glassful of raw gin on going into bed, "just to make her sleep," as she mildly expressed it. On making this discovery, regarding her habits, I own that I was candid and foolish enough to pronounce, on grounds moral and physical, a strong disapproval of the practice in question. What the effect of this lecture was upon the

patient I had no opportunity of knowing, as I was never asked to renew my visits. *Verbum satis sapientibus!*

6. There is not much, I think, to be learned from observation of the pulse in these cases, unless we happen to see the patient when under the stimulating influence of drink, at which time the pulse will be found greatly accelerated, even though the patient may be far from intoxication. This vascular excitement, without any morbid lesion to account for it, would then become a very suspicious circumstance. Most observers agree in representing the pulse to be rather slow and feeble during the intervals of intemperance, but yet not exhibiting any pathognomic character.

7. There is a group of nervous symptoms, two or more of which are pretty constantly present in chronic alcoholism. For reasons already mentioned, I only advert to these in a general way, just to complete the enumeration of symptoms which may attend upon chronic alcoholism, and thus aid in its recognition. This group comprises the following: extreme nervousness; sleeplessness; fidgetiness; muscular tremors; hallucinations of sight or hearing; and epileptiform seizures. Of this last symptom I have only met with one example. The attacks had come on after the patient—a married lady aged twenty-five—had been tipping for two years. She presented many of the other symptoms of alcoholism. I may just mention she was almost the only patient I ever met who confessed—though not before she was charged with it—to being guilty of intemperance.

A word as to muscular tremor. Some writers regard this as the most constant and reliable sign of alcoholism. It is true we may have the same symptom arising from mercurial intoxication, from paralysis, and from pure senility; so that, *per se*, the tremor is not pathognomic of alcoholism. Nevertheless, the alcoholic tremor generally admits of easy recognition. It may amount to a state of universal rigor, or show itself in the unsteady gait, or faltering prehensile power; it can be suspended under the influence of strong maniacal excitement, and persist during extreme agitation; and sleep has much less effect in stopping it than in stopping the movements of chorea. It is not so apt to accompany alcoholic delirium coming on in the course of another disease, as when this delirium attacks a person otherwise healthy.

Among soldiers a common test of a man's sobriety is his ability to hold the right index and thumb steadily apart at a distance just sufficient to let the light be seen betwixt them and no more.

Notwithstanding this long list of symptoms, it must be confessed that many cases will come before us where we may suspect intemperance, but yet find it hard to prove it. Nay more, chronic alcoholism may exist and yet not reveal itself by any symptom. This latent alcoholic saturation, as Trousseau calls it, possesses peculiar interest for the physician, as it may declare itself by its effects at a critical moment when the patient

is attacked, for instance, with some acute disease, and then the alcoholism shows itself by an unexpected delirium or by an ataxic condition out of all proportion to the severity of the organic disease.

I fear I have trespassed too long on the time of the Association. Let me now bring this crude, ill-digested paper to a close, but not before I ask your indulgence towards its many faults, of omission and of commission.

THE CHAIRMAN looked upon Dr. M'Clintock's paper as one of the most interesting communications that had been laid before the Society for a long period. It dealt with a very obscure subject, and one that delighted in secrecy, and the more information they obtained upon it the more likely were they in the practice of the profession to render service to their patients. He, unfortunately, had some extensive observations of the effects of intoxicating liquors, from being connected with a lunatic asylum, and he could bear out what Dr. M'Clintock had said—that these were cases which had organic origin, and had led to that miserable and degrading vice. He could not say that it was confined to any particular class of females, as far as being married or unmarried was concerned; but it was generally to be found among the unmarried females who had arrived at a certain age. It would appear that, when the most important function of the uterus was not fulfilled, there was then a disposition for the reception of stimulants, and these cases presented the greatest difficulty in being relieved. He, however, had one case under his care in which the patient evinced the worst possible form of addiction to this vice, and, after being in the asylum for some time, she became reformed, and for four years had remained perfectly well, and in mental health, and never returned to her former vice. These cases were therefore not to be looked upon as incurable.

DR. STEWART was glad to hear the President say he had seen one recovery in the case of a female. He (Dr. Stewart) had had a large experience of lunatics, and had seen many painful cases of alcoholism among them; but he had never met with one instance of the recovery of a female.

DR. JOHN EUSTACE did not think he had ever listened to a paper with more interest than he had done to Dr. M'Clintock's valuable communication; but there was one view of the question which he rather expected Dr. M'Clintock would have directed his attention to—and with which he was particularly qualified to deal—namely, with regard to the hereditary transmission of the disease. Having met with a great number of these cases (on account of being a specialist in some degree) the impression had been made on his mind, from the history of them, that

almost the largest proportion of these cases could be traced back to hereditary transmission. He thought the view which had been brought before them, with regard to the peculiar odour of the breath, was one of great importance. It was a feature in those cases which he had invariably observed, but he did not agree with Dr. M'Clintock that the patients desired to conceal their breath from the observation of the physician, for they had sometimes cast it upon him as a boast, saying, "you will get no smell of drink off my breath;" and the fact was as they said. He remembered a medical man of some eminence in this city who was under his care some years ago. In that case his attacks presented all the appearances of fever, and for a number of years, when abroad and travelling in England, he had been treated by some of the most eminent medical men for fever. He happened to come to some Dublin doctors, who, knowing more about the matter than he supposed, decided that their poor brother's case was one of intemperance. When placed under his (Dr. Eustace's) care, they trusted somewhat to his honour not to drink, and the consequence (as might indeed have been expected) was that he had an attack of fever. He had the tongue and the pulse of fever, but there was the peculiar ethereal odour from his breath that indicated the real cause of his illness. Yet he declared, with tears and oaths, that he was suffering from fever. He (Dr. Eustace) had a most careful search made, and even when the brandy bottle was produced, not half empty, as in the case described by Dr. M'Clintock, but altogether empty, he declared it was a false charge against him, and threatened an action for defamation of character. That gentleman invariably appealed to his breath to show that he had not taken drink. There was another point he wished to mention. With regard to the treatment of these cases he had found that it was best to discontinue the poison as soon as possible; and if the patients could be induced to take plenty of food they would soon be got all right. There was a morbid appetite for food in many of these patients, and if the patient could be got to indulge in it, it might take the place of the appetite for drink. In the Swedish legislature advantage had been taken of that fact, and publicans there were licensed on condition that they would supply cooked food to all persons frequenting their houses, they having a profit on the cooked food and the state having a profit on the drink. The publicans, therefore, in their own interests, became advocates of temperance. He thought the Society ought to unite to put pressure on Dr. M'Clintock to investigate the hereditary tendency of the disease, which is quite a matter within his province.

DR. H. KENNEDY thought that, in these cases of alcoholism, there was something in the constitution of the individual which led to the vice. Every one must be struck with the fact that persons equally exposed to

temptation would not all fall into drinking habits; and precisely the same was true of tobacco; every one could get these things, but all did not choose them. Hence, he thought it was something peculiar in the temperament of the individuals that led them to drink. A deranged nervous system might lead gradually to the use of stimulants; but there was something totally independent of that; for, in the case of females, it could not be supposed there was such a strain on their nervous system as to lead to those habits. Therefore, it appeared to him, that there was a certain kind of constitution which had a tendency to run to drinking. His experience of the administration of wine and spirits to patients in hospitals did not show that it had led those persons into bad habits. The poorer classes who were treated in hospitals did not like wine, and if they did, could not get it; but, he thought, he had known some instances among people of a higher class in society, where the ordering of stimulants as medicine had led to drunkenness. With regard to Dr. Eustace's remarks of the hereditary character of this complaint, he believed there was sufficient evidence to show that a drunken father was likely to be succeeded by a drunken son. On many occasions, more formerly than of late years, he had committed the mistake of overlooking cases of this sort. The tippling was generally so well concealed, and the surrounding circumstances were so totally inconsistent with the idea that the individuals would fall into drink, that the physician should ever be on his guard. The point of the latency of the effects of alcoholism was important. Persons addicted to this vice often went on for a long time apparently well until they meet with some trivial accident, when the wound or general symptoms assumed a form that showed the latent alcoholism.

DR. NIXON said, as regards the symptoms and signs so valuable in the diagnosis of alcoholism, he wished to ask Dr. M'Clintock if he recognized another one—he alluded to a peculiar œdematous condition of the face, a flabby condition of the muscles. He (Dr. Nixon) had noticed this frequently at the hospital and dispensary, and from it arrived at a conclusion, which proved to be correct, that the patient was the subject of chronic alcoholism. The muscles of expression were entirely lost, and there was that flabby condition of the face so often seen in the debauchee.

DR. AQUILLA SMITH said there was nothing more universally admitted than the fact that, in our time, intemperance had greatly increased among the better classes of society. His friend, Dr. M'Clintock, however, did not enter into a consideration of the causes of that undue amount of intemperance. It was a good while ago since he read a paper upon the improper use of alcoholic drink, by an eminent physician, Dr. Rush, who flourished at the close of the last century. He recollected distinctly that

Dr. Rush, who was admittedly the most eminent practitioner in America in his time, had foreseen the danger that might arise from medical men sanctioning the use of whiskey as a remedial agent in the treatment of patients. He did not admit at all that the physician, by ordering stimulants, led to habits of intemperance, but he foresaw that the charge might be made against them; and they all knew it was the common dodge among the better class addicted to immoderate indulgence in drink, to say that their physician had ordered them to use stimulants freely, and that this had led them into the habit. He did not believe the medical profession was greatly to blame in the matter; and he looked upon these statements as a mere device which these people had adopted in order to screen themselves from the imputation. He had met with several instances where that was the apology the drunkard made for his indulgence in this vice. Among the causes which had led to intemperance among the higher classes was the total change of habits. In his early days there was no such thing known among young men as soda-water and brandy, and sherry and bitters, and he saw too many instances now of the evil of indulging in these stimulants frequently during the day. The habit had probably come from America, and was one of the causes of the great increase of intemperance. He was old enough to recollect when the habits of the better classes were very different from what they now are. When a young man he had known of parties where the door was locked and no one allowed to go out until he had drunk as much as the host thought proper. He knew many persons who lived to be 70, 75, and 80 years of age, who enjoyed good health and were active vigorous men, and yet were in the habit of drinking many tumblers of punch every day, but they never touched a drop before dinner. It was well known that the danger to health was greater by taking small quantities of spirits frequently than by drinking occasionally to excess. He would succeed, in time, in killing any man if he gave him a tea-spoon of whiskey every hour in the day, whereas four tumblers of punch after his dinner might not do him any harm. The frequency of taking small doses of spirits was one of the great causes that led to organic disease. He believed there was the peculiar odour in the breath of drunkards referred to by Dr. McClinton; but his (Dr. Smith's) suspicions were more frequently raised by finding a smell of carraways and peppermint in the patient's breath. The mendacity of the victims of alcoholism was notorious, and they often showed wonderful skill and ingenuity in obtaining drink. The other day a man was admitted to Dun's Hospital suffering under painter's colic. He thought, or said that he thought he was labouring under a difficulty of passing water and asked for some gin to enable him to do so. He (Dr. Smith) refused to give him any, and said he would order him a mixture that would have the desired effect, and when the man found he could not get the gin his kidneys began to act well. The

mendacity displayed by these people was a consequence of the demoralized state they had been brought to by an insatiable thirst for intoxicating drink. As to the circular to which Dr. M'Clintock had referred, he felt ashamed of the medical profession falling into such a trap, and he thought many had signed to get rid of the importunity of the persons who hawked it about, and who published it to serve their own objects.

DR. FINNY wished to call attention, as an aid to diagnosis, to the connexion which intemperance bore to insanity, or such diseases as were due to nervous disorganization. Some members of a family he attended had shown the symptoms of alcoholism, while others had shown nervous diseases, neuralgia, epileptiform convulsions, and dementia. He had to deal with these cases rather as a disease of the mind than a vice of habit. One point among ladies who suffered from various nervous symptoms he had noticed to be the great relief they derived from tea and such like stimulants, and he found that very often they varied the tea with champagne, and in this manner produced symptoms of alcoholism. In one of these cases both husband and wife were affected. The wife called on him one day to know how to overcome the vice of the husband, she herself having been under his care from the effects of the same thing! He had often noticed the smell on the breath of such patients, but he did not know its cause or its value as a diagnostic until now, and he had, therefore, derived that information from the present discussion. He thought the idea that alcohol prescribed as medicine had been the cause of bringing on this vice had been most unfairly urged, and in many instances made a screen behind which to hide themselves by habitual drunkards. He believed the true safeguard against such a danger in prescribing stimulants was to order them only to be taken along with either medicine or food.

DR. M'CLINTOCK thanked the Society for the kind way in which they had received his paper. The subject was one of the most extensive kind, and the difficulty was to confine himself to any one branch of it, and to resist the temptation to follow out others which presented themselves for consideration. He might have adduced a great many clinical illustrations of chronic alcoholism and its results, but it would have extended his paper to undue length. He knew that in bringing the subject forward in any one shape he could not fail to elicit much interesting discussion, and he was glad the speakers had not confined themselves to what was the special drift of his communication. It had often been stated that though there is hope in reclaiming a male drunkard, you will never reclaim a female. He had, however, known one or two cases of women having been reclaimed. One lady had gone very far. She then got a very serious illness from which she narrowly recovered, and from that time

she had completely given up drink, and now enjoys good health. It was, however, almost a solitary instance in his own experience. With regard to the hereditary transmission of the propensity to intemperance, that was a subject which, in our own day, had engaged a great deal of attention, and it seemed to be very generally admitted, that if we look into the history of persons who were drunkards, we shall find that the propensity had shown itself in some of their ancestors. As to what Dr. Eustace said about drunkards asking the physician to smell their breath in proof of their sobriety, the fact was, that sometimes they were not themselves aware that their breath had the peculiar ethereal smell, or that it indicated habitual intemperance, and appealed to the test, knowing that the smell of raw spirits would not be got from them at the time. A strong odour of carraway seeds or of cloves (as Dr. Smith mentioned) was also a very suspicious indication, being almost always taken to conceal the smell of spirits or tobacco. As to the puffy condition about the eyelids, referred to by Dr. Nixon, that was more to be found in confirmed drunkards where alcoholic cachexia had come on, and no doubt it was a striking feature in the alcoholic physiognomy, and was generally accompanied by œdema of the conjunctivæ. Although it was possible some blame might occasionally be attributed to medical men for incautiously prescribing stimulants, and thus creating an appetite for them, yet we should not accept the statements of drunkards themselves on this point, as their mendacity is too notorious! Often a stimulant was ordered by the medical man to overcome some degree of weakness or sinking, and patients very soon found out that a little brandy, or whiskey, or wine, was equally effectual in remedying that distressing prostration or sinking. Of course the more frequently they took these diffusible stimulants the more necessity they felt for resorting to them. In most of these instances it was not for the mere pleasure of swallowing these stimulants that they were taken, but to remove an overwhelming feel of depression or sinking. As to the *causes* that led to excessive drinking, the question was a very interesting one. He only alluded to one of them—the intense intellectual activity and competition of these times. He must, however, admit with Dr. H. Kennedy that this would not so strongly apply to the cases of intemperance among women. The question would afford ample scope for a special inquiry, and to the philanthropist, the legislator, and the physician it was one of the deepest importance. He did not touch in his paper on *treatment*. He thought, however, it was a great mistake not to discontinue completely and entirely the taking of spirits, in all cases of alcoholism. He believed no injury would arise from totally and abruptly withholding it from the patient.

The Society adjourned.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIFTH ANNUAL SESSION.

JOHN R. KIRKPATRICK, M.B., Honorary Secretary.

DR. SIBTHORPE, Vice-President, in the Chair.

Saturday, March 8th, 1873.

DR. HENRY KENNEDY, in the absence of Dr. G. Beatty, brought before the meeting a boy of twelve years of age, who was affected from birth with spina-bifida. The boy had grown with his years, was well made, and healthy in every respect. His head was not too large, nor had he ever had convulsions. The tumour, about the size of an orange, is situated in the lumbar region, and has the appearance as if it were slightly narrowed, as it emerges from the spinal canal. It is tense and diaphanous, and has a distinct impulse communicated to it by coughing. The coats seem very thin. Direct pressure on it gives the boy at once the desire to micturate. If the pressure be lateral and on each side, he shortly gets heavy and stupid, and there can be little doubt, if pushed further, would cause convulsions.

The case caused a good deal of interest amongst the members present, and it seemed to be the very general feeling of all, including Professor Robert Smith, that no operative proceedings should be put in force at present.

On Dactylitis Syphilitica ; a Specific Affection of the Fingers and Toes. By J. MORGAN, M.D., F.R.C.S., Professor of Surgical and Descriptive Anatomy, Royal College of Surgeons, Ireland; Surgeon to Mercer's Hospital, Dublin, &c.

DR. MORGAN said that having met with some cases of inherited constitutional syphilis peculiarly affecting the fingers and toes, he thought it desirable to bring the subject before the Society. In this peculiar affection it would seem that the earlier stage of the syphilitic evidence was rather skipped over, and the case glided at once into the tertiary condition (assuming that this affection represented the tertiary stage of syphilis). The vast majority of children show the syphilitic affection within the first month; but this peculiar affection was one that came on at

indefinite periods. He exhibited a cast taken from the hand of a patient, aged forty, a married woman, who suffered from dactylitis, a disease which had been particularly described by Drs. Taylor and Parry, and previously to them, by Chassaignac and by Nélaton. This woman was infected by her husband some years ago, and at present she was suffering from gummatous ulcerations, known as a consequence of syphilis, and had suffered for nine months from this peculiar affection of the fingers. It got better for a time, but he learned from her husband that day, that the disease had recurred. In that case, and in those represented in the drawings he exhibited, the disease did not affect the deeper structures, but the connective tissue around the joint. There was a mild affection, where there was a swelling of the fingers, which gave a creaking and semi-elastic feel, and was not painful; but then there was also another form, when the disease became more painful and more distinctly coloured on the surface. Dr. Morgan exhibited casts and drawings illustrating this form of the affection. One case was that of a child, the produce of parents that were affected by syphilis. The mother had given birth to two healthy children; she was then syphilized by her husband, and produced this child. The cast showed the globular form assumed by the part affected, presenting the appearance as if the thumb were thrust through an Indian-rubber ball. After some time it became coloured on the surface, and by-and-by it assumed a very tense appearance, became a purplish kind of red in colour, and when opened gave exit to a thin fluid, and underneath the gummatous matter was seen. It might be supposed at first that this was not a case of inherited syphilis, but there was well-marked evidence of its being so, as a well-formed syphilitic rash showed itself. He treated it with bichloride of mercury and bark in small doses, and applied iodized flexible collodion to the part, and it took seven months from the time it first formed till the sore healed. This child suffered from an affection of the foot and of the thumb, which showed the peculiar globular appearance. It was the same with a child whom he had attended along with Dr. Cronyn. Both the child's feet had the violet or purplish tint, and the tense and fluctuating feel. One swelling resolved itself without being opened; the other was opened. Dr. Cronyn was sceptical at first of its being a syphilitic case, but a rash appeared ere long which made them have recourse to specific treatment, under which the child recovered. The case was one of much interest, as Dr. Cronyn had already attended the mother in two previous confinements, when she gave birth to healthy children, but this, the third child, suffered from dactylitis, without having had snuffles, or any of the earlier evidences of being syphilitic. Another case which had come under his notice was that of a boy nine years of age; he was scarred all over the body from the marks of ulceration he had had from time to time, and on the back of the hand he had this peculiar swelling. An

incision was made, and the fluid let out, and the gummatous matter was found underneath. These instances he regarded as typical cases of the disease. First they had it in a woman of forty, not very long affected, and then in children varying in age from two to five and nine years. The most interesting case, however, he had seen was that of a boy of eleven, who was one of the greatest sufferers from inherited syphilis that he had ever met with. He had eight marks of gummatous suppurations from time to time. The bones were not diseased, but at the same time he had an enormous gummatous abscess in the ham, and several scars over his body; in the next hospital bed to him there was a man suffering from a gummatous tumour, and the appearance was the same as was found in these gummatous children.

The question would arise, was this a syphilitic symptom? Some might think it scrofulous. The history of the case, however, and the other symptoms, such as the appearance of the syphilitic rash, put the matter beyond question. It was seven years previously to his birth that the mother was affected; she had a child two years older than the boy, and this child had syphilitic symptoms also. That seemed a long time for the syphilitic taint to remain in the mother, but they should deal with the facts as they found them. It might be said that in young children they would not have the gummatous stage; but he now exhibited a drawing, showing syphiloma of the liver, the appearance being as if little grains of hominy or *semola* were scattered over it, and they were connected with other syphilitic signs. This was the child of a mother in the hospital affected by primary syphilis, which soon ran into a terrible gummatous inflammation, and broke her down. Six weeks after birth the child got a gummatous ulcer of the ear, so that in that case there was a sudden transition from the earliest stage to the tertiary stage. In adults the same thing was seen, the gummatous stage occasionally taking the place of the secondary. He exhibited another drawing, showing the peculiar appearance of gummatous tumours, on the liver of a gentleman who, eighteen years previously, had contracted syphilis in China; he appeared free from symptoms for some time, but afterwards his health broke down, and he got syphilitic disease of the larynx, going down to the bifurcation of the bronchi. He (Dr. Morgan) laryngotomized him, with some relief for a short time, and after death his liver was found to present a very perfect illustration of the deposit of gummatous matter. With respect to where the dactylitis occurred, it was generally in the first joint of the finger, and on the dorsal rather than the palmar aspect. The skin was at first whitish, very tense, and had a creaking sensation. By-and-by it assumed the peculiar livid appearance represented in the drawings, and might give way. There were two points which he would briefly allude to in connexion with inherited syphilis. First, as to the vexed question of the

influence of the father or mother in producing syphilis. From one of the instances he had already mentioned they might draw some conclusion. In the case which he saw along with Dr. Cronyn the father admitted that he had been affected. Dr. Cronyn attended the mother at the birth of the two former children, and neither of them showed any taint whatever, and the mother remained healthy; and yet here was a child presenting symptoms two years after birth, showing that it carried the syphilitic disease until at length it manifested itself in its feet.

It was a disputed point whether the father could produce a syphilitic child without influencing the mother? He believed the father could so affect the child without affecting the mother; but the influence of the mother beyond yea or nay was very potent. He saw a case of a mother producing a healthy child, and the child remaining healthy for two years. The mother was one of the worst cases of syphilis in the hospital, and was under treatment in it for two years, and was ultimately carried off by small-pox. The child did not show any sign of syphilis until the end of two years, and then it showed severe syphilitic taint.

As to the treatment of dactylitis, it yielded under anti-syphilitic treatment, and bandaging the part, or mixing a drachm of tincture of iodine with an ounce of flexible collodion, and at the same time giving bichloride of mercury. It seemed to him that many cases supposed to be scrofulous affections were in reality syphilis cases, which would yield to bichloride of mercury, with tincture of bark. A finger might be amputated under the belief that it was affected by scrofula. The feet, of which he had exhibited drawings, were greatly diseased; the swellings were most alarming in appearance, yet it was wonderful with what rapidity they yielded under mercurial treatment. He thought, therefore, the subject was one worthy of being brought under the notice of the Society, as containing many points of both theoretical and practical interest.

DR. CRONYN had not much to add to Dr. Morgan's description of the disease, which he had faithfully and accurately given; but as affecting the practical view of this question, he would make a few observations. He attended the mother of the child, who produced two apparently healthy children, and this was the third. The two others he had seen before the birth of this child, and frequently since. The woman had since been confined of a fourth child, which, so far as he was aware, had remained for months in a healthy condition. Thus before the birth of the third child and since she had borne healthy children. Whether the fourth would develop syphilitic symptoms remained to be seen, but it was now nearly a year old. The third child was under his care for the affection of its foot, and he suggested the desirability of a consultation, and Dr. Morgan was named to him. He had not the slightest suspicion that the disease was of syphilitic origin. On the contrary, he imagined

it to be strumous. Dr. Morgan told him his view of the case, which, he confesses, startled him, and in which he was by no means inclined to agree. He told Dr. Morgan that he was so much occupied in observing syphilitic diseases that the subject had become a hobby with him. However, when the father admitted he had had syphilis he (Dr. Cronyn) began to see that Dr. Morgan was on the right road, more particularly so when he (Dr. Morgan) showed him several cases of a similar character then under his care. One of these swellings in his (Dr. Cronyn's) case was opened by him, and a small quantity of glairy fluid was discharged. The other was treated by the administration of mercury and iron, and went on very well. There were still some appearances about the feet, but they had returned to their original formation, and the child run about, and was perfectly healthy, active, and strong. It was under treatment three months from the time Dr. Morgan saw it first.

DR. M'SWINEY said, as far as he understood Dr. Morgan, he seemed to question the circumstance of a syphilitic child being capable of being born to a father tainted with syphilis unless he affected the mother; and then he admitted that such cases were to be met with, but were extremely rare. He (Dr. M'Swiney) thought it would be in the memory of many of them that the late Dr. Beatty long since opened an inquiry on that subject, which was followed up by others, the result being that it was now an undoubtedly established fact that a syphilitic child might be born of a mother who had never had syphilis, in consequence of a syphilitic taint in the male parent, and that a mother might be affected with syphilis by a syphilitic child she was carrying, without having any ulcers on the organs of generation. Obstetrical practitioners were in the habit of meeting with cases of dead children succeeding one another in the same woman, these children having signs of being dead for some time. Investigations had established, he believed, conclusively, that in these instances the child was dead because it had suffered from syphilis, and the children continued to be born dead until the parents had been subjected to a course of mercurialization, as recommended by Dr. Beatty. Other cases had shown that the male parent might carry about with him a syphilitic taint which affected his offspring, and that after going through a process of mercurialization healthy children were born to him. It was an established fact that the semen of a parent affected by syphilis was capable of producing a child affected by syphilis, such infant being capable of infecting the mother.

DR. HENRY KENNEDY said that the tendency to set down different diseases to the effects of the syphilitic poison was at present a remarkable feature in the profession, and particularly in London. He could give cases from the weekly periodicals as having been brought before the

Pathological Society there, which seemed to him to be essentially strumous disease, and not the result of syphilis. He did not question the facts which Dr. Morgan had brought forward this evening; but he did think they might bear a very different interpretation, and that it was much more likely they were examples of strumous disease. Dr. Morgan had stated, as an argument in favour of the disease being syphilitic, that the discharge from the swellings which had been opened or burst was very thin, and not at all like what occurs when struma exists. Now, with all respect for the author, there is nothing more common than this in cases which are essentially scrofulous. Again, the author had argued that because mercury had been of benefit the disease must have been of syphilitic origin. Now, here again Dr. Kennedy would observe that mercury, gradually administered, was often useful in strumous cases, and as an example in point he might mention strumous ophthalmia, in which the bichloride proved so frequently superior to any other plan of treatment. But again, the very long intervals which had elapsed in some of the cases given between the supposed period of infection and the appearance of the syphilitic symptoms was an additional reason for questioning the author's views. He was aware that this poison might lie long latent, but such cases were exceptional, and would but badly support the cases which had been detailed, some of them running out, if he rightly understood the author, to twelve, eighteen, and twenty years. Dr. Kennedy, too, could have wished that drawings of the eruption which the author described as occurring in some of the cases had been shown to the Society. In the last place Dr. Kennedy spoke of the work of the late A. Collis, in which the effects of the one poison on the other, and of the two combined, were discussed with great acumen by that distinguished surgeon.

DR. MORGAN, in reply, said he had seen acres, he might say, of syphilitic rash, and therefore ought to know by this time what it was. Although he had only spoken of the syphilitic rash appearing on the leg in one of the cases referred to, the fact was that the body of the child was covered with it from head to foot, and one of these children had a syphilitic patch on the side of the mouth. He had not the slightest doubt that all the cases he had adduced were of syphilitic origin. He had treated them freely with mercurial enunctions, whereas if they were scrofulous children he should not like to give them mercury; but under that treatment the disease yielded. He considered that mercury must be looked on as the only anti-syphilitic remedy that was yet known.

On the Treatment of Vesico-vaginal Fistula when the Urethra, Neck, and Floor of the Bladder have been Destroyed. By GEORGE H. KIDD, M.D., F.R.C.S.I., President of the Dublin Pathological Society; Ex-President, Dublin Obstetrical Society; Hon. Fellow, London Obstetrical Society; Corresponding Member of the Gynæcological Society of Boston, and of the Obstetrical Society of Berlin; Obstetric Surgeon to the Coombe Lying-in Hospital.

The treatment of vesico-vaginal fistula has now arrived at such a degree of perfection that there are but few cases in which a cure may not be accomplished. For a fistula of a moderate size the paring of the edges and bringing them carefully together is generally sufficient; for those of a much larger size, accompanied with protrusion of the fundus of the bladder, the splitting of the edges and bringing the flaps together by the quilled suture, as recommended by the late Maurice Collis, has in my hands been the most successful mode of treatment. The very small fistula, so small as to resemble a pin-hole, the closing of which one would think ought to be a very simple affair, has often given me more trouble than openings through which I could easily pass one or two fingers; but since the adoption of the flap operation, its cure has become most certain, simple, and easy. We have had twelve of these cases in Dublin, each cured by a single operation, and there have been no failures. In nine of these cases I operated myself, my colleague, Dr. Roe, in one, and Drs. Mapother and Bennett one each.*

The case to which I wish at present to draw attention is one where the difficulty arises from the total loss of the base and neck of the bladder, and of the urethra, where, in fact, the whole of the anterior wall of the vagina has sloughed away, from the neck of the uterus to the symphysis pubis, converting the bladder and vagina into one cloaca. For the treatment of this condition two methods have been suggested—one by Jobert de Lamballe, who proposed to make an opening into the rectum, and then close up the vulva completely. In this way the urine and secretions of the vagina and uterus must pass into the rectum, and be discharged with the fæces, the three canals being converted into one great cloaca, and the woman brought, by the attempts of art to repair

* Since this paper was read I have received the following letter from Mr. Smith, of St. Bartholomew's Hospital, London. Mr. Smith's cases make a total of sixteen, cured by a single operation and without one failure:—

"5, STRATFORD PLACE, W., 17th March.

"DEAR SIR,—You were so kind some time ago as to put me in possession of the details of your operation for closing small vesico-vaginal fistulæ by covering them with a flap of mucous membrane. I write a line to let you know I have had occasion to put your plan into practice on four occasions, and in every case successfully. Two of the cases had given me much trouble, the old operation having failed to cure them, though I had performed it with all care on several occasions. With many thanks,

"Believe me to be, yours truly,

"George H. Kidd, Esq., M.D."

"THOMAS SMITH.

the great misfortune she has suffered, into a state resembling the normal condition of birds.

The advantage of this operation is of course that it makes use of the sphincter ani to supply the place of the sphincter of the bladder, and so gives the woman the power of voluntary control over her excretions. The disadvantages of the operation are so apparent as to make it almost unnecessary to allude to them. In the first place, it unsexes the woman. There is great danger of hæmorrhage from wounding the hæmorrhoidal vessels in the operation; the opening into the rectum is very liable to close by contraction of the cicatrix, and fæces may pass into the vagina and bladder, and form the nuclei of calculi, or set up a severe form of cystitis.

To the late Mr. Isaac Baker Brown we are indebted for suggesting another operation, one which he himself performed several times, and which has frequently been performed by other surgeons. Mr. Brown proposed to make a new urethra by passing a trocar or small knife through the tissues under the arch of the pubes in the direction in which the urethra formerly ran. In the canal thus formed he placed a catheter, which he kept there till the parts were all healed. By these means he hoped to have a permanent canal formed. As soon as he believed he had this accomplished, he proceeded to make a new floor for the bladder by drawing the uterus down and the sides of the vagina together. Mr. Brown gives the details of several cases in which this operation was quite successful, and gave the patients complete control over the contents of the bladder. But it was not always so successful. During one of my visits to Edinburgh, I saw with Sir James Simpson an American lady who had come over to England to be treated by Mr. Brown. She had had fourteen operations performed. The fistula was closed, and a new urethra formed, but she was really no better off than before the operation, for, owing to the absence of a sphincter muscle, she could not retain water, notwithstanding all she had gone through. Sir James Simpson was trying to fix an Indian-rubber ball so that it would press on the new urethra and close it, but at the time of my visit his efforts had not been attended with success.

Dr. Emmett of New York has described two cases in which he made new urethras, but in the first the patient could not retain water, nor could she completely empty the bladder; and after being under treatment for some four years the fistula opened again from the accumulation of calcareous matter in the bladder, and all further attempts at cure were given up. In the second case the result was more fortunate. After about three years' treatment and some sixteen operations, the patient returned home able to retain water, but not able to completely empty the bladder without the aid of a catheter.

Dr. Deroubaix of Brussels describes two cases in each of which he

made a new urethra. He followed Baker Brown's method more closely than Emmett, but the results were not very encouraging. The first patient was under treatment from October, 1865, to December, 1867. She underwent some nine or ten operations, many of which occupied from two to three hours, and at the end of all she could only retain water by the use of a spring compressor applied to the urethra, and she could only empty the bladder by the use of a catheter. The other patient was under treatment, with intervals more or less prolonged, from March, 1865, till July, 1868, and also underwent repeated and tedious operations, and at the end could only retain water by the aid of a compressor. To empty the bladder completely, and also prevent the new urethra from closing by the contraction of the cicatricial tissue forming its wall, she had to pass a catheter at short intervals. The merits and demerits of this operation may be summed up, as I have already done, with that of Jobert de Lamballe. Its merit is that it restores the bladder and urethra, while it leaves the vagina intact, and the patient may, as Dr. Deroubaix remarks, enter society, re-engage in her occupations, and even marry, the only inconvenience being that she should use a catheter every time she might require to completely empty the bladder. In this enumeration Dr. Deroubaix makes no mention of the patient's having to wear a compressor to enable her to retain her water, or of the length of time she had to be under treatment, or of the many very serious or dangerous operations she had to undergo.

In the case I have to bring under your notice the patient was enabled, by a very simple operation—one as devoid of danger as any surgical operation could be, one occupying but a very brief time in its performance, and completed at one sitting—to retain water perfectly by the use of a compressor analogous to that used by Dr. Deroubaix. She could empty the bladder completely without using a catheter. She could thus re-enter society, and resume her occupations, but she could not marry or enter into the relations of married life.

This circumstance was fully explained to the woman before the operation was undertaken, and she at once stated that she was not married, and would gladly forego marriage for the sake of the other benefits to be derived from the operation. In another case subsequently under my care, the patient, who was a married woman, and already had had eleven children before the accident occurred, declined to pay the penalty the operation would exact for the benefits it could confer. But in the case of an unmarried woman, with a deformed or undersized pelvis, and who has already suffered injury from a difficult labour, the being able to marry is a very doubtful advantage.

An unmarried woman of low stature, deformed from rickets, and of a low order of intellect, came under my care in the Coombe Hospital in the early part of 1866, suffering from incontinence of urine, the result of a

tedious and difficult labour. On examination I found a large opening from the vagina into the bladder, which extended from the os uteri to the commencement of the urethra, and from the ramus of the pubes of one side to that of the other. On the right side the margin was formed of bone covered only by periosteum and mucous membrane. The vagina was filled by a large round tumour of a bright scarlet colour, which was the remaining portion of the bladder inverted and protruding through the opening in its floor.

I made several attempts to close the opening and made such progress that I came to what I thought would be the final operation which was to crown my efforts with success. But whether owing to the previous long confinement to hospital and consequent deterioration of the woman's health, or other causes, the operation utterly failed; the vaginal portion of the neck of the bladder sloughed away, and the urethra with it. There was not sufficient tissue left for Baker Brown's operation even if I had been inclined to do it, and for a time I thought the case utterly beyond the aid of surgery. At length it occurred to me to close up the vagina altogether, leaving a long narrow passage close to the pubes which might answer the purpose of the urethra; and I had some hope that by making the canal as long as possible and so narrow in its diameter that it would require some force to make the water flow along it, the adhesion of its sides from capillary attraction would be sufficient to retain the water except during expulsive efforts.

After explaining what I proposed to do, and obtaining the patient's consent, I proceeded to the operation.

The patient was put under the influence of chloroform, and placed on her back in the lithotomy position. I then pared the mucous membrane off the inner surface of the labia, and continued the dissection round the posterior wall of the vagina, which was the more easily done as the anterior part of the perinæum had been torn. In making this part of the section I went as far up into the vagina as I could, so as to avoid having a pouch posteriorly in which a portion of urine would be likely to lie after the remainder had been passed. Anteriorly I removed the nymphæ, and then laying a No. 10 gum elastic catheter close up under the arch of the pubis, I removed as much of the mucous membrane as I could, leaving only the portion under the catheter and which was to form the lining of the new urethra. The pared surfaces were next brought together by the quill suture. The threads were passed through the whole thickness of the labium. Thus the operation resembled that usually performed for the formation of a new perinæum. The needle used was that generally known as Liston's—that is, a needle in a fixed handle with the eye near the point. Having been threaded it was first passed through one labium from the skin to the mucous or inner surface. It was then withdrawn, leaving the thread in its place. It was now armed with another thread

and passed through the other labium from its inner or pared surface to the skin, and withdrawn, leaving its thread. The ends of this second thread were now passed through the loop of the first which was drawn back carrying the second thread with it. This thread now passed through both labia and was ready to have the quills applied. By this means the sutures were passed with more accuracy than could well have been done otherwise, and they were placed close to the deep edge of the wound. Four sutures were inserted and tied over a piece of Indian-rubber cord, taking care to bring the deep surfaces of the wound into contact. The superficial edges were brought together by interrupted wire sutures. A catheter was placed in the track left for it, and the patient put to bed. At the end of forty-eight hours the quilled sutures were removed, and at the end of ten days the wire sutures. The whole extent of the wound united by the first intention, and as long as the patient lay quietly in bed she could retain water perfectly, but when she was up or making any exertion the water escaped by the newly-formed urethra from the want of a sphincter muscle to control it. I therefore determined to get a spring made which might compress the urethra, and act as an artificial sphincter, after the plan of one described by M. Trélat as having been made for a patient in the Maternité de Paris by M. Charrière et Fils. This woman had had a portion of the urethra destroyed, and the orifice of the remaining portion rendered so patulous that she could not retain water. M. Trélat twice tried to narrow the orifice, but failed, and then applied to M. Charrière, who, after several attempts, made an instrument that enabled her to have perfect control over the water. This instrument is described and figured in the *Gazette des Hôpitaux* for the 30th December, 1865, and Messrs. O'Neill and Thompson, to whom I showed the drawing, made a similar one for my patient. It consisted of a steel band, like a truss, covered with leather. This band encircled the pelvis, the ends being fastened over the sacrum by a strap and buckle. From the middle of the band, corresponding to the pubes, an arm ran downwards for a short distance, to the end of which a spring was attached by a pivot-screw. This spring was curved, so that its free end, on which an olive-shaped piece of hard wood was fixed, pressed against the under surface of the new urethra. When the instrument was in its place the woman could retain water perfectly in any position; she could walk, she could run, and she could carry a weight without any escape. When she wished to empty her bladder, she turned the spring aside, which the pivot enabled her to do, and when she had done she replaced it. A very slight amount of pressure was found to give her perfect control over the escape of the water. She remained under observation for several months, and then left Dublin to go to America, and I have seldom met a patient more grateful than she was for the relief she had obtained.

This instrument is apparently identical with that described and figured by Dr. Deroubaix, who, however, does not say whether he copied it from that of Trélat or designed it himself. It is, however, worth noting that the description of Trélat's instrument was published on the 30th December, 1865; that the instrument for my patient was first applied on the 20th of December, 1866; and that for Dr. Deroubaix's patent in the early part of December, 1867.

DR. CRANNY, in the absence of Dr. Johnston, who had been called away, related the case of M. H., aged thirty-six, who had been admitted to the Rotunda Hospital, suffering from great incontinence of urine, which, on examination, was found to be in consequence of complete absence of the anterior wall of the vagina, and, of course, posterior of the urethra, from sloughing, the result of tedious labour in her last (the ninth) confinement. In fact there was not a particle of urethra left but a thin ring, the thickness of a piece of whip-cord, at the meatus urinarius. She was in such a deplorable condition that the only position she could remain in at all with ease was standing, resting on a table or chair, with her legs apart, and thus allowing the urine as it was secreted to drop on the floor.

Considering her pitiable state, and that an operation, even though it failed, would not render her condition worse, the whole vulva was closed up by means of wire sutures by Dr. Johnston, as far as the clitoris, much in the same manner as Dr. Kidd had so clearly described, leaving an opening sufficient to allow a No. 11 catheter to pass. The parts healed completely, and when the cicatrix was sufficiently hardened, a species of truss compressing the urethra, which had thus been formed, was tried, in order to act as an artificial sphincter. This not succeeding to our satisfaction, an elastic band with a cork pad was applied which seemed to answer better; but being anxious to return to the country, she left before we could perfect the instrument—not, however, before, as she said herself, she was, comparatively speaking, in the greatest comfort, being able to lie in bed, even without the instrument, quite dry, and by keeping a napkin tightly applied to the urethra in the day time, she could sit and walk about with but little escape of water.

DR. ATTHILL said the case brought forward by Dr. Kidd was another proof of the great advance made in the surgical treatment of this disease. His case was an eminently unpromising one—a great cavity existing, no tissue out of which to form a urethra; therefore, he must look on the result of the case as a great triumph. He (Dr. Atthill) had a very similar one. The patient was an unmarried woman who had been delivered by the natural efforts after a very tedious labour. The vagina was closed above by a firm band which occluded the uterus, a very small opening only

existing, through which she menstruated. From below this band to within a very short distance of the orifice of the urethra, the entire vesico-vaginal wall was wanting. About half an inch of the urethra remained, but it was impervious. The first step was to endeavour to re-establish the urethra. For this purpose a trocar was thrust through it, and a catheter subsequently retained in the passage thus made, and thus a permanent canal was established. He then performed an operation similar to that described by Dr. Kidd, but instead using the quilled suture brought the pared edges together by means of wire stitches, but on removing these, after the lapse of some days, he found that the vulva were united for hardly one half of their extent. As soon as the parts were healed he repeated the operation, and this time used the quilled sutures as well as the ordinary wire sutures outside; and the operation had perfectly succeeded. The woman had returned to her home. She could empty her bladder, and although at present that was a tedious process, he anticipated in course of time that she would be able to do so with facility. The case was not so unpromising as Dr. Kidd's, inasmuch as there was a portion of the urethra left which they were able to utilize.

DR. M^CSWINEY asked, did Dr. Kidd say that this was an operation which he would recommend?

DR. KIDD—I recommended it to Dr. Johnston, and he followed the recommendation. I believe it is the best operation that can be performed under the circumstances. It puts the woman, at a very small expense of suffering and time, in the best position she can be under her unfortunate circumstances.

DR. ATTHILL said that in his case the woman was young and unmarried, but had borne a child. He told her she would be unsexed, and she willingly submitted to the operation.

The Society then adjourned.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

Dr. KIDD, President.

Pericarditis.—DR. WALTER SMITH said the specimen he exhibited was taken from the body of a strong, robust man, who was admitted to the Adelaide Hospital in the beginning of last September, and died a month after his admission. The case presented some features of interest and some degree of obscurity during life. The patient was a man of temperate habits, and had always been in the enjoyment of good health. In the middle of last July he lost his appetite, suffered from thirst, and was troubled with flatulence. His health broke down in a few days, and he was unable to continue at his work. There was no history of rheumatic fever, or of any other acute illness; but dyspnoea, oppression, and tightness of the chest soon set in, followed by dropsy, and three weeks before admission his legs swelled considerably. When he came in, his state was one, chiefly, of marked and extreme debility, with troublesome dyspnoea and rapid pulse, averaging from 112 to 120, and respiration persistent at about 40: during the whole time he was in hospital it was never under that figure. The heart's sounds were extremely feeble, and at one time were scarcely audible, but no murmur or any physical sign of valvular mischief or inflammatory disorder of the pericardium was at any time perceptible. The urine was scanty when he came in, from eighteen to twenty ounces per diem, but it was not albuminous, and under the influence of digitalis and iron soon rose from twenty-one to seventy or eighty ounces. The dropsy increased, his respiration became almost entirely phrenic, lividity of the face supervened, delirium set in, and he died in four weeks after his admission.

On *post-mortem* examination, there was a great deal of fluid found in the abdomen, but the only noteworthy appearances were those connected with the chest. The lungs were greatly congested and cedematous, the condition which immediately led to death. On opening the thorax a great deal of fluid escaped, which came from the pericardium. The outer surface of the pericardium appeared like the anterior surface of the heart, it was so smooth and uniform. The pleura was adherent to it and dotted over with specks of fibrin.

When the pericardium was opened it presented the following appearance:—It was extremely thickened, the thickness in some places reaching to one-third of an inch, and thickly coated over on the greater part of its surface with a closely adherent layer of soft lymph, rugose and honey-

combed like the gall bladder. On the anterior and inner surface there was a large intensely injected patch.

The surface of the heart itself was covered with the same yellowish fibrinous covering as was found on the interior of the pericardium. The weight of the heart and its appendages was 2 lbs. 8 ozs., and when removed from the body and held up in the hand it fell over and covered the hand like a mushroom cap. The diameter of the heart, vertically and obliquely, was seven inches. On cutting into the structure of the organ it was seen that the right auricle had scarcely a trace of muscular tissue—the scanty muscular fibre stood out in relief on a back-ground of yellowish fibrin. The left auricle was thin, and the muscular tissue was soft and friable, and of a light chocolate brown colour. The left ventricle almost pitted on pressure.

The most important point in the case was the insidious supervention of this extreme form of exo-cardiac disease, without any history of rheumatism or other apparent cause that might have led to it. The man only came under medical observation at a time when all the physical signs of an early stage of disease had subsided, and when he presented only the symptoms of a weak and dilated heart. He himself felt all through that he would not recover, and shortly before his death expressed his conviction that he was dying. The pericarditis probably had its starting point in last July, was then unsuspected, and remained without treatment, and when he came into hospital, the condition of the muscular tissue of the heart was so far affected that permanent benefit from treatment was out of the question.—*December 7, 1872.*

Encephaloid Disease of Thigh.—MR. JOHN HAMILTON said:—A man came to the dispensary of the Richmond Hospital for a swelling of the lower part of his thigh. His history was the following:—He was a coach painter, aged thirty-three. Seven months before, he had fallen down and hurt his knee, and for a few days he was lame. After that he continued to attend his work as usual, occasionally suffering from stings of pain in the knee, but nothing very remarkable. Two months before admission and five months after the original accident he perceived a swelling at the lower part of the thigh, and this was accompanied with a certain degree of numbed pain. The swelling became so great, and the pain and the weight of the limb so distressing, that he could not go on with his work, and he walked to hospital, and applied for admission. When Mr. Hamilton saw him in bed, he appeared a man of sallow complexion, but not otherwise unhealthy looking, with a tumour occupying the lower third of the thigh. It was of a pale colour, perfectly uniform on the surface, and of the same temperature as the surrounding parts; there was no tenderness, nor was there more than a slight, dull pain. The tumour appeared to be fixed to the thigh, and had an obscure sense of fluctuation—it was like fluctuation

deep between the bone and the periosteum, with the soft parts covering it. However, it was not such, nor was the man's suffering of that degree as to induce him to make any puncture to let out matter. On the contrary, there was something in the feel of the tumour and the general aspect of the case to make him hesitate. It was not malignant in its appearance, as it was not lobulated, nor was it discoloured, nor were there any large veins on the surface. He waited from day to day, watching the progress of the tumour. It was very slow, and the man himself was not urgent to have anything done. On that day week, while he was turning in bed, he felt a sudden snap, attended with excruciating pain. He sent for the resident surgeon, Mr. Thompson, who saw him lying on the same side as the diseased limb, the knee considerably swollen, and the man crying out from pain. The slightest attempt to move the limb gave him great agony. The next morning Mr. Hamilton saw him, and found him just in the position Mr. Thompson had described, and the least attempt to move the limb gave him great pain; the swelling of the thigh had increased, the knee was also swollen, and the man begged of him not to touch it, he suffered so much pain. The tumour felt less fluctuating than before, but he thought it right to make an exploratory puncture, as it might relieve the suffering. Accordingly, he made an opening with a French bistoury, but nothing issued save blood. He then passed in a director; it moved freely in some soft material, and when removed there was some grumous matter, not unlike cerebriform tissue brought with it. He then made up his mind that the tumour was malignant; but what was the state of the knee which was so tense and swollen, and what was the cause of the excruciating pain that came on so suddenly? There was some discharge of grumous fluid from the opening, but not much. The next morning after the accident the thigh was not swollen, but it afterwards swelled greatly up to the groin, the swelling altogether masking the tumour. He was taken ill on the evening of the 30th with this violent pain, and he died on the evening of the 3rd of the following month, the fourth day.

When Mr. Thompson made the *post-mortem* examination and cut into the thigh, there was a quantity of dark, turbid red fluid, infiltrated through all the cellular tissue of the limb. When the tumour was opened into, it was found to be an encysted growth of large size, composed of encephaloid structure. The femur was in the centre of this malignant mass. It had become considerably diseased. The laminated structure of the bone had been absorbed in several places, and, the absorption going on within as well as without, there was but a mere shell left, and when the man turned in bed this broke across. It appeared to him that after this fracture of the bone ordinary inflammation of the osseous substance took place, and that this extended to the knee. The joint was full of bloody

fluid, and the condyles of the femur, the patella, and the synovial membrane were all of an intense red colour from inflammation; and moreover, in some places there was ulceration of the cartilage, both of the patella and of that covering the condyles of the femur. His own belief was that this was of an acute character. There was no indication of ulceration of the cartilages before, for he had had no great pain, nor any of those painful startings peculiar to that affection. There was a case recorded by Mr. Lawrence, of the occurrence of ulceration of the cartilages in four days, and this he believed was another case of the same kind. Its interest therefore was very great. It was an instance, not very often met with in malignant disease, of a fracture of the bone occurring without any violence. Some years ago he exhibited a case of fracture of the humerus from malignant tumour in an old man, who broke it simply by leaning on his elbow. The rapidity with which the disease progressed was also remarkable. Another point of interest was that the man could go on walking about, following his business, and even walking to hospital with his limb in such an advanced stage of disease, for it was only within a couple of weeks after coming into hospital that the diseased bone broke across.—*December 7, 1872.*

Malignant Tumour in the Abdomen.—DR. HEAD exhibited a tumour taken from the body of a child aged seven years. The first time the tumour was observed, the child called attention to it itself in the month of September last. It was then about the size of an orange, and occupied a space a little below the left hypochondrium, but extended up under the ribs. The child came under Dr. Head's care in the Adelaide Hospital on the 19th October, and then the tumour was nearly the size it was at present. It increased very slowly until the child's death on the 28th of November. The interest of the case lay in its diagnosis, and also in the occurrence of so large a malignant tumour in so young a child. It had a sickly appearance, and there was a slight ecchymosis under one of the eyes. It was able, however, to eat and drink very well, and there were no cerebral symptoms, which he drew attention to because it was found there was disease in the skull. The question was, whether the abdominal tumour was disease of the spleen or of the kidney? The fact of the large intestine passing the point of the tumour made him come to the conclusion that it was not one of the spleen, and rendered it probable that it originated in the kidney. He thought it possible the tumour was malignant, but a fortnight after admission a tumour appeared under the right orbit, and when he found it had the elastic character of fungus-hæmatodes, he came to the conclusion that it was malignant, and considered it a growth secondary to the abdominal tumour. It sprang from the upper plate of the orbit and appeared on the upper surface. The bone was not penetrated, but was evidently infiltrated; there were several

patches of infiltration in the skull. Mr. Ball, having examined it under the microscope, came to the conclusion that it was of the kind known as round-celled sarcoma. On cutting into the abdominal tumour, no doubt could be entertained of its malignancy. The spleen was connected to it, but only by a peritoneal attachment. This organ was healthy, as also was the liver. It was thought at first that it sprang from the kidney, but on dissecting that organ out of its capsule, it was found not to be attached to the tumour, but it was greatly compressed and the seat of fatty degeneration. The difficulty of diagnosis in these cases of tumours loose in the abdomen, arose from the fact of their being behind the intestine, and the crossing of the intestine in front of them.—*December 7, 1872.*

Disease of the Prostate Gland.—DR. WHARTON exhibited a specimen of diseased prostate gland, which, with the bladder and organs of generation, had been removed from the body of a man, aged seventy-five, who had recently died in the Meath Hospital. He had frequently been under his care at the above institution on account of retention of urine from enlarged prostate. The symptoms of retention were well marked, but the bladder was never distended, and the *post-mortem* examination showed that this viscus was of small dimensions. There was no difficulty experienced at any time in relieving the patient by means of the prostatic catheter. On the 27th of the last month he was placed under Dr. W.'s care, under circumstances plainly to be understood from observing the enlargement to which the gland had attained, and particularly its middle lobe. Previous to his admission into the Meath Hospital, he had been an inmate of one of the many charitable institutions with which this city abounds, where he had been treated for his usual symptoms. On these occasions an ordinary, not a prostatic catheter, had been employed. On the fourth day after his last admission to hospital he died, somewhat unexpectedly, and the *post-mortem* examination was made about thirty-six hours afterwards. It will be seen there was no stricture from the external meatus to the neck of the bladder, on the contrary, the urethra was large and distensible. It is questioned by anatomists whether there exists in the natural state a third or middle lobe in the prostate gland. In the preparation before the Society there can, indeed, be no doubt of its presence, for it is as large as a medium-sized grape, and its base is traversed by a false passage. As before remarked, the bladder is of small dimensions; its mucous membrane is thrown into rugæ, the free borders of which are ecchymosed. The consistence of the gland is soft, and it is infiltrated with pus. The ureters were not dilated.—*December 7, 1872.*

Coagulation of the Blood in the Heart.—DR. HAYDEN brought before the Society an example of sudden death by general coagulation of the

blood in the heart. The patient was a man thirty-three years of age, an engine-driver, and very intemperate. He was admitted to the Mater Misericordiæ Hospital, on Tuesday, 3rd December. He had been ill for some months previously, his symptoms being those of general debility with cough. Dr. Hayden saw him on Tuesday morning; the pulse was then rapid but weak—there was no febrile action, the skin being cool and the tongue clean. The heart acted very feebly, the impulse being scarcely perceptible. The man had cough with mucous expectoration, and an examination of the chest satisfied him that there was disseminated tuberculosis of the right lung. He prescribed in a very general way, deeming the case not an urgent one, and intending on the next day to make a more careful examination, but on going to the hospital next morning learned that the man had died suddenly and unexpectedly. At five o'clock, a.m., his resident pupil was hastily summoned to him, and found him gasping for breath, with a very rapid and feeble pulse; the surface was cold and remarkably pallid; he breathed with great difficulty, very rapidly and very deeply, and in 15 minutes from the first accession of these symptoms he was dead.

As he had determined, the right lung was somewhat solid; it was encased in a dense false membrane, its apex was corrugated, and two or three small cavities were found in it, besides some detached cheesy nodules in the central and lower portions. Through the left lung a few disseminated tubercular masses were found. There was some serum in the cavity of the right pleura. The pericardium contained seven or eight ounces of serum. The heart was remarkably pallid, with a good deal of superficial fatty deposit. Its cavities were all nearly plugged with firm, decolourized fibrin, which extended from the right auricle through the tricuspid orifice, rendering the tricuspid valve incompetent by holding it widely open. The right ventricle was dilated. The left side of the heart was likewise thrombosed, but the left ventricle was in a state of so-called *concentric* hypertrophy. They would observe, passing from the solid mass of fibrin in the left ventricle, and extending through the aorta, and into its primary branches, especially the left common carotid, in which it was very thick, a solid cylinder of fibrin. In the pulmonary artery was also found a mass of a similar kind, and thoroughly decolourized. He had not traced it into the lung, but had no doubt it would be found ramifying throughout the branches of the pulmonary artery. Under the microscope the muscular substance of the heart was found to be in a state of advanced granular degeneration. Both kidneys were greatly enlarged, and presented good examples of the waxy or amyloid condition. Their section resembled the cut surface of a white turnip. The Malpighian bodies were magnified, and over the field were seen floating particles of broken up fibrin, and disintegrated epithelium. The weight of the kidneys was 9 and 9½ ounces respectively.

This case was of interest chiefly in regard to prognosis. There was nothing in the condition of the man, as cursorily determined, to warrant the apprehension of his early and sudden death; but if he had been able, by a careful examination, to determine the state of the heart, connecting that with the difficulty of circulation through the lungs, he should have been a little less confident. Cases of this kind ever and anon occur. He thought in most cases the heart would be found either in a state of granular or fatty degeneration—if, in addition there be serious impediment to the circulation through the lung, as from bronchitis, and, at the same time, emotional depression or excitement, slow coagulation of the blood in the right chamber of the heart, with separation of fibrin, might be reasonably apprehended. The thrombosis in this case was general, and this led him to believe that disease of kidneys had a great deal to do with the result. He believed, if the blood were examined, there would be found an excess of white corpuscles, constituting a condition of leucocytosis.—*December 7, 1872.*

Empyema communicating with the left Psoas Muscle, and passing thence into the Vertebral Canal.—DR. FOOT laid before the Society the viscera of a boy, aged ten, in whom an empyema of the left side had passed by a fistulous communication behind the internal arched ligament into the left psoas muscle, in which it made an abscess, and from thence into the vertebral canal. The empyema was a primary, spontaneous, latent empyema; when the boy was sent to hospital it was for paraplegia, which was accompanied with paralysis of the bladder and incontinence of fæces: these symptoms had occurred rather suddenly. He lived nineteen days after the appearance of the paraplegia, passing ammoniacal urine, and suffering from bed sores. After death the exterior of the theca vertebralis was found thickly covered with adherent, brownish lymph, mixed with yellow patches of purulent matter; the inner side of the dura mater spinæ was quite unaffected, as also were the other membranes and the substance of the cord. The deposit on the theca extended as high as the brachial enlargement.

It was supposed that the pus gained admission into the spinal canal through the intervertebral foramina which give exit to the branches of the lumbar plexus. The fistulous communication between the psoas abscess and the left pleura was plainly seen, and admitted of the passage of a small catheter.—*December 7, 1872.*

Hepatic Abscess.—DR. FOOT exhibited the viscera of a case in which an hepatic abscess had opened into the right pleura, and death had resulted in twenty-eight hours after the perforation. The subject was a man thirty-five years of age, a workman in a distillery, who was admitted

into the Meath Hospital 18th November, 1872, with the symptoms and physical signs of recent pleuritis on the right side. He had been four days under observation when he was suddenly seized with acute pain in the lower part of the right side and most aggravated dyspnœa. He lived for twenty-eight hours after the time these violent symptoms set in.

The body was examined in thirteen and a-half hours after death. Clear yellow serum sprung out as soon as the right costal cartilages were divided, the boundaries of the right side of the chest were covered with exudation fibrine of greyish colour, in sheets or agminated dots. As the liquid contents of this side of the thorax were removed, it was seen that the fluid consisted of various strata; uppermost a clear serum, then an opaque puriform liquid, and deepest of all was a reddish stratum, with a good deal of blood corpuscles and blood colouring matter in it; the mixture of the three produced a thick chocolate-coloured inodorous product, such as would be called "grumous." The right lung was shrunken, shrivelled, wrinkled, sheeted with greyish exudation fibrine, through openings in which the darker grey of the pulmonary substance here and there appeared; the lung itself was only compressed, there was no exudation into its parenchyma, it was readily inflated, and on doing this the deposited lymph split and cracked in all directions. The opposite lung was free from adhesions, engorged with blood, and its pleural sac contained ten ounces of reddish fluid, no doubt the result of the great collateral hyperæmia. On the floor of the right pleural cavity there was found an irregularly oval ragged rent in a part of the diaphragm which was formerly adherent by its under surface to the upper border of the liver; this aperture was the mouth of a large hepatic abscess which had sloughed through the diaphragm—the cavity of this abscess would have held an orange; a second abscess, not communicating with this, and a little smaller, was deeply seated in the right lobe, just below the other one; and a third, also isolated, the smallest of the three, also in the right lobe, was found a little to the left of the other two. The contents of the second abscess were of two kinds—what first came out when the fluctuating prominence indicating its situation was cut into was glutinous and glairy, like the fluid of a ranula, or like water arrowroot; the remainder of the matter was a light greenish yellow pus, free from smell. There were no other purulent collections in the liver. The organ weighed 101 ozs. (6 lbs. 5 ozs.) Its measurements were as follows:—The greatest length of the right lobe was nine inches, and its greatest breadth seven inches; the length of the left lobe was seven inches, and its breadth four inches, and thus the breadth of the entire organ was eleven inches; the greatest thickness of the right lobe was four inches. The parenchyma of the liver was smooth and pale, and the organ had the microscopic appearances of a fatty liver, which were fully borne out by the microscopic examination, which showed the liver cells, while quite fresh, to be

infiltrated with fat granules and globules to a very high degree. There was no perihepatitis with the exception of that in the immediate vicinity of the abscess which had opened into the pleura, where there were many adhesions between the convex surface of the liver and the diaphragm. The gall bladder had no calculi, was moderately distended with a thin orange-coloured bile, no ulceration of its inner surface or of the biliary ducts. A small fibrinous coagulum lay loose in the portal vein, and bifurcated into its two primary branches; the branches of this vein were free from ulceration. The spleen weighed $6\frac{1}{2}$ ozs.; one thickened white patch on its convex surface indicated previous perisplenitis. The colour was carefully examined, and it exhibited no appearances of an abnormal kind, nor did any of the glandular structures connected with it. There was no abdominal nervous congestion, nor any fluid in the cavity of the peritoneum. These abscesses possessed several of the principal features of tropical abscesses, although there was no history of the man's residence abroad. Their size and number, the bursting into the cavity of the pleura, and the absence of jaundice distinguished them from pyæmic abscesses; there was no history of an injury or contusion of the liver, no evidence of interstitial hepatitis, ulceration of any of the canals, or of the appendages of the liver, no indications of their origin from tuberculous deposit or hydatids, and so Dr. Foot considered that, well-marked as the specimen was as one of hepatic abscess terminating in a most characteristic manner, it must yet be added to the list of cases of this affection, the etiology of which is most obscure.—*December 7, 1872.*

Disease of the Bladder and Kidney.—DR. M'SWINEY said a man was brought, on last Tuesday morning, to Jervis-street Hospital in a dying state. The people who accompanied him were either unwilling or unable to give any detailed information as to his previous history. All he could learn was, that he was supposed to have had rheumatic fever; that he had been ill for a week—got worse—and that the people where he lived thought it better that he should be removed to hospital. When Dr. M'Swiney saw him, shortly after his admission to hospital, he was evidently dying. He was in a semi-comatose condition; his features were drawn and shrivelled; he was cold, pulseless, and gasping. He died in the course of the day, and on the following morning a *post-mortem* examination was made.

Hearing that he was supposed to have had rheumatism, it was thought right to examine the organs in the thorax in the first instance, but information of a negative kind only was obtained by the inspection of the heart and lungs. The liver was next examined; it was apparently quite healthy. Now, he should before have stated that when looking at the external surface of the body he had observed that there were several cicatrices in the

situation of the perinæum. This region had been evidently the seat of operative interference at some remote period; it was hard, nodulated, irregular, and bulging, and had two or three old marks of cuts; in like manner there were the deep lines of two former incisions, one on either side of the scrotum, and the testicles were firmly adherent to the surrounding parts—all circumstances plainly indicating that there had been, at some previous period, an extravasation of urine with operative interference to relieve it. He presumed there had been at that time an impermeable stricture of the urethra. The seat of the disease which caused the man's death was found to be the urethra; it was the subject of old disease. The genito-urinary system, beginning with the bladder, was carefully examined. The condition of that organ was peculiar. It was tensely distended, and the patient had retention of its contents, but owing to the extreme smallness of the organ, an examiner could not arrive at that fact by palpation during life. The ureters were greatly enlarged, and each curved in a very tortuous manner to its respective kidney. The kidneys, like the bladder and ureters, were distended with fluid. The bladder, when opened, was found to contain a whey-coloured, purulent liquid having no smell of urine; in like manner, the ureters were filled with the same kind of fluid. The bladder was unusually contracted, its substance immensely thickened; it was greatly corrugated on its internal surface, which was of a dark blue colour. The kidneys presented an example of the most entire degeneration—indeed destruction—of the renal structure he had ever witnessed. They were filled with the yellowish, whey-coloured fluid already mentioned, and were completely excavated in the parts corresponding to the site of the pyramidal bodies—each pyramidal body being in fact replaced by an abscess. The cortical structure round the edges was for the most part destroyed, and the pyramidal structure had entirely disappeared.

There were many features of interest connected with this case, but the prominent one was, that it afforded an extreme example of the fearful ravages that a disease beginning, insidiously, by an impediment to the free flow of the urine, was capable, ultimately, of producing. The sequence of events seemed to be: First, a stricture (probably from gonorrhœa) extravasation of urine, continued irritation of the urinary organs, inflammation of the bladder, extending along the ureters to the kidneys, purulent formation, and, ultimately, death from the absorption into the system of the excrementitious matters which it was the function of the kidney in health to eliminate.—*December 14, 1872.*

TRANSACTIONS OF THE CORK MEDICO-CHIRURGICAL SOCIETY.

President—DR. FERR.

Hon. Secretary—DR. HOLMES.

Report on Small-pox. By H. MACNAUGHTON JONES, M.D., F.R.C.S.I., &c.
Extraordinary Physician, Cork Fever Hospital; Senior Demonstrator,
Queen's College, Cork; Surgeon, Cork Ophthalmic and Aural Hospital;
Ex-Physician, Cork City Dispensary.

MR. PRESIDENT AND GENTLEMEN—When I undertook to furnish for this evening (at a few days' notice), a report on the small-pox epidemic which has passed over our city, I did not, I confess, weigh as carefully as I ought to have done, the entire responsibility of the task I had undertaken. I set out with the determination to make that report as brief as possible, to avoid any allusions to established facts; in short, to place before the Society such statistics and peculiarities of type of this late outbreak as I could gather together, in so short a time, and then to ask the members individually to give such additional information as would make the report worthy of this Society. I find on examination that but few facts in connexion with this disease can be said to be *established*. I need only ask you to peruse the discussions of the Medical Society of the College of Physicians within the past year to satisfy yourselves on this point.

The utility of re-vaccination, the contagious properties of the disease, the treatment, the very nature of the disease itself, were all points which on discussion elicited opinions widely differing in the most important and vital particulars. Now there are present men who, from their experience of past epidemics, their well-known character for close and careful clinical observation, and their rigid desire for strictly truthful representations of clinical facts, are as well calculated to throw light on the few doubtful matters (if indeed they can be called doubtful), which have led to discussion elsewhere, as any in the United Kingdom. Therefore it was that I came to the conclusion that it was the duty of all of us, if our Society is worthy of its name, to join in making this report on small-pox a valuable report, and not the mere subject of a few minutes' interesting conversation. I have felt, I assure you, my utter incapability, in the short time I had to work, to do anything like justice to a subject of so grave a nature.

The statistics I have to present you with are of so meagre a character as hardly to warrant my calling them statistics; yet even they have their value.

For my own part I do not place that importance on long statistical tables that some do. I would look rather to sound views of treatment, the carefully recorded occurrence of unusual symptoms, the establishment, by repeated personal observation, of contested, yet all-important facts in connexion with this disease.

For these reasons I forwarded to thirty medical gentlemen in this city a number of queries as follows :

" 1. What is your experience of the effects of vaccination ? (a) On the intensity of the symptoms, and if it exerts a modifying influence ;—when and how was this most shown in the progress of the cases under your care, as contrasted *with* a non-vaccinated case ? (b) On the duration and sequences of the disease. (c) As a preventive ?

" 2. What is your experience of re-vaccination *as a preventive* ? Have you had any instances of unpleasant results from the operation ?

" 3. Has the past epidemic afforded you incontestable proofs of the *contagious* nature of the disease ?

" 4. What have been the peculiar features, if any, which struck you as present in this epidemic, as compared with past ones ?

" 5. What have been the complications which were more usually present ?

" 6. What unpleasant sequences have followed in some instances ?

" 7. What is your experience of antiseptics in treatment ?

" 8. What would be your opinion on the advantage of a *Tent System of Hospital* in the treatment of small-pox ?

" 9. If a Dispensary Physician, would you kindly give the total number of cases which occurred in your district, and the date of the first case in your register ? (a) The total number of deaths registered. (b) Your opinion, formed from careful observation, of the effects from overcrowding and bad sewerage on the mortality ; if possible, also, a brief example of these latter. What, in your opinion, mainly contributed to the spread of the epidemic in your district ? Are you of opinion that the sanitary precautions adopted by the authorities in your district, were instrumental in checking the progress of small-pox in it, and of such a character as you, in your capacity of medical officer, approved of ?

From the majority I have received replies, and I shall presently give you some details of that evidence which I look on as of greater moment than any statistical summary. I must again ask you to excuse many shortcomings in this report. It is neither as full of statistical evidence or results of clinical observation as I would desire ; yet if it did no more than provoke such a discussion, as I trust it will in this Society, we will be amply repaid. When I see those members who are present this evening, I feel an assurance that so important a subject as small-pox will attract attention it demands.

Bacterial cholera, small-pox is the greatest of human scourges,

if it is not equally dreaded. To investigate the causes, symptoms, and management of such a disease, must be the noblest and foremost work of any Medical Society.

It is to perform that work creditably to ourselves, and in keeping with the character of our Society, that we are met together this evening.

I propose, in the first place, to lay before you such statistics as I have been able to collect. Secondly, to invite your attention to the characteristic features of the epidemic, as seen by myself and the other members of the profession in Cork. Lastly, to put forward certain propositions of importance in reference to small-pox, and to invite an unprejudiced discussion of these.

I may here say that it is not my intention to draw into this paper any opinions outside those of the members of the profession in our own city. I shall avoid all allusions to authorities or other statistics, as I believe we all have ample data from which to draw as correct conclusions as any elsewhere, and we are met not to enter into the deductions and statistics of others, but those derivable from the local outbreak.

The first table I will ask your attention to is one which gives the daily returns and total number of cases, as inspected by the Corporation officials from reports furnished to them by the medical officers of the dispensaries, and returned by the Street Inspectors, from the outbreak of the epidemic to its close.

The first case of small-pox which showed itself in this city, occurred in Evergreen-street, and was reported on the 8th of December, 1871.

I shall allude to this case more fully presently. From this table it appears that during the months of April and May the epidemic reached its height, the greatest number returned on any given day being 85 on the 30th of April; nor can we say that there was any perceptible lull until the 18th of June, from which date the numbers contrast strongly with those of the corresponding days in May. In July the epidemic began to decline rapidly, the greatest number reported on one day, the 8th, being 12 cases.

For my own part I do not place that importance on long statistical tables that some do. I would look rather to sound views of treatment, the carefully recorded occurrence of unusual symptoms, the establishment, by repeated personal observation, of contested, yet all-important facts in connexion with this disease.

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Ages at which Death took place.

1	1 to 2	2 to 5	5 to 10	15 to 20	20 to 30	30 to 40	40 to 50	50 to 70
9	4	7	8	1	3	3	2	2

Of these there were

Vaccinated,	-	-	7
Not vaccinated,	-	-	18
Doubtful,	-	-	14
Total,	-	-	39

Over 66·0 per cent. of the deaths registered were in children under ten years of age.

I had in my dispensary district 587 cases. The deaths registered were 100. Many of the cases, however, were removed to hospital, and hence the number of deaths registered was no criterion of the mortality, nor indeed in any given district outside, as the deaths which occurred in public institutions were registered in the district in which these were situated.

The following are the small-pox statistics of the Cork Union Work-house to the 6th September, 1872 :—

Classification of Ages	Vaccinated		Not vaccinated	
	Admitted	Deaths	Admitted	Deaths
Under ten years, - - - -	147	16	235	126
Ten and under fifteen, - - -	202	9	78	13
Fifteen and upwards, - - -	405	17	164	86
Total, - - - -	754	42	472	225
Total number, - - - -	Admitted	Deaths	Mortality	Percentage
	1226	267	Vaccinated, -	6·89
			Not vaccinated	56·81
			Average, -	21·70

I now come to treat of the epidemic from my own personal experience. Small-pox practically was to me a new disease. Having seen only one case previous to this outbreak, I could judge of this epidemic only as contrasted with the description of the disease as I had read of it. I have heard a great deal said, and I know that there was a popular notion prevalent in this city, that the epidemic bore many unusual and startling features not presented by former ones; but I must confess that my experience only verified my former conviction—a conviction shared in, I am sure, by many here, which is, that if you wish for a clear, practical, and vivid description of the symptoms of the disease, you will find that description in the pages of Watson. If you look at the account given in this work of the small-pox epidemic, as seen by Sir Thomas Watson in 1838 in London, you must admit that it exhibits no wonderful difference in any particular from the one we have witnessed. Contrast the disease as there depicted with the symptoms described by Niemeyer in the comparatively recent work by this justly celebrated pathologist, and you will find very little practical difference.

The progress of the disorder, the complications, the sequences, all are mentioned by Sir Thomas Watson as having occurred in the other epidemics.

Take, for example, the hæmorrhagic variety, which we all so justly dreaded; you will find it alluded to by Sir Thomas Watson. He says:—

“In one most fearful phase of this always formidable disorder, symptoms indicative of what is called putrid diathesis manifest themselves—petechiæ vibices, hæmorrhages from various parts of the body. The pustules, instead of being plump and yellow, are flat, red, purple, or blue; that is, they contain blood or a separate sanious ichor in the place of pus, constituting variolæ nigræ of Sydenham, the bloody small-pox of Mead. I believe that these appearances augur in all cases a fatal result. Hæmorrhage from the uterus is not uncommon, and in pregnant women abortion, and then most commonly death.”

Or again, “the rose-coloured rash,” which has been described as preceding the eruption, and to which Drs. O'Connor and Donovan have drawn attention. We find he describes thus:—

“I may mention, also, that the eruption is not unfrequently preceded by a rash something like that of scarlet fever, and called by Willan the ‘Roseola Variolosa.’ It fades in the course of a day or two, and then the small-pox pustules are seen emerging in the same state that they would have been in if no such rash had appeared.”

He draws attention to the fact that the rash occurs more frequently in the inoculated, when it may be looked on as a favourable sign; a dark red colour, and in the casual disorder, it is considered to be of a severe confluent attack.

I think we might describe the epidemic which has just to

as a malignant type of varioloid, characterized by its rapid progress, universal spread, and the absence of those disfiguring scars and cicatrices which remained after previous epidemics.

The absence of pitting which has been generally noticed after this epidemic, is accounted for by the fact that in varioloid the deeper layer of the skin or the *cutis vera* is not involved. A curious instance of this occurred in my private practice; a gentleman rather vain of his personal appearance, with very sallow skin and dark hair, having passed through a light attack, which resulted in mild pustules that attacked the cutis, exposing the pigment cells, was greatly troubled at the dark spots which covered his face, and remained for some time after the attack had passed away. He attributed the staining to the solution which I had used to cover his face during his illness; this of course all passed off after a time. The points of practical interest which have struck me in this past epidemic are in their order of importance—

- 1st. The undoubtedly contagious nature of the disease.
- 2nd. The good effects of vaccination.
- 3rd. The value of re-vaccination.
- 4th. The large numbers of vaccinated affected.
- 5th. The absence in a large number of cases of a *good cicatrix* in persons who had been vaccinated.
- 6th. The malignant nature of the disease in a large number of cases.
- 7th. The generally severe type of the disease in plethoric and fat or robust patients.
- 8th. The generality of the throat and chest complications.
- 9th. The peculiar delirium.
- 10th. The terrible purpuric cases.
- 11th. The few ophthalmic complications.
- 12th. The apparent want of unanimity on the part of the profession regarding treatment, the value of the stimulant plan, and the powerlessness of drugs of any kind to produce visible or material results during the progress of the disease.
- 13th. The universal spread of the disease amongst all classes.
- 14th. The universal sequelæ, abscesses and boils, with occasional joint affections.
- 15th. That small-pox is a disease for nurse-tending, requiring above all things a skilled and superior class of nurse.
- 16th. The use of disinfectants in purifying the air of apartments, and nullifying the effects of poisonous emanations from patients and their discharges.
- 17th. The urgent necessity existing for compulsory legislation on the removal of pauper small-pox patients, as also on the sanitary powers of corporate and other bodies during such an outbreak. The sanitary precautions taken during the past epidemic being to my mind (with the

exception of the whitewashing and cleansing of lanes and houses which was often most imperfectly carried on), in the majority of cases utterly worthless, and calculated to do more harm than good, by lulling into a sense of false security the ignorant amongst the lower orders, who fancied that the sprinkling of a little chloralum solution on their floors was a talisman against the further inroads of the disease.

18th. The questionable propriety of establishing small-pox wards or departments in close connexion with or proximity to other hospitals or institutions.

Contagion.—These particulars regarding the first few cases of small-pox which occurred in this city establish, I think, the contagious nature of small-pox.

The first case of small-pox occurred at 26, Evergreen-street. Timothy Driscoll, a native of this city, came from Newport sick, in one of Mr. Abraham Sutton's coal vessels, on 10th November, 1871, and had nearly recovered on the 8th December, when the case was reported with three others; one from the same house; another, a niece of Driscoll's, who frequented the house during his illness, and in whose home Driscoll's wife slept while he was sick; the fourth person reported on that date lived opposite Driscoll's house. Driscoll's mother sold coal on the premises, so it is very probable that they all had recourse to the house, as they were ignorant of the nature of his complaint.

Driscoll recovered, and is now in America. Ellis, a child from the same place, died in the Union Hospital on the 28th December, 1871. The other two recovered; one, in the Fever Hospital, named Egan, and the other named Bease, in the Union Hospital.

I had myself frequent proofs afforded me of the contagious character of the disease, of which I have no doubt whatever. I think the facts put forward in the replies I received in reference to this point will completely satisfy you.

Every physician who has answered query No. 3 did so in the affirmative, with the exception of Dr. Curtis, who, it strikes me, has received erroneous information regarding the commencement of the epidemic, which may have influenced him not a little in coming to his conclusion. Dr. Cummins, on the other hand, adopts the view that, "although small-pox is contagious, contagion alone is unequal to causing a general spread of the disease when epidemic influence is wanting."

The well-known fact that the disease spread in, and was carried to the country districts after the Cork races on the 15th and 16th of May, shows the manner in which the disease is propagated. That epidemic influence exerts its own power is also beyond a doubt, as I suppose there is not one listening to me who has not seen cases the presence of the disease in which could only be accounted for by such epidemic influence. I attended two children out of a family of six people, who lived in an

isolated part of my district, the cabin in the midst of fields, and accessible only by means of a long lane; the children were not once in the city; the father or mother seldom; yet these two took the affection and recovered: no one else in the house was attacked, though the parents would not remove them to hospital. I have seen instances where persons slept in the same bed with small-pox patients, yet escaped; mothers carrying children in their arms and nursing them without any result to themselves.

There must be then, I think, a third factor, to account for the universality of this disorder, that is, the presence, from various hidden causes, of a certain susceptibility on the part of the individual for the reception of the poisonous influence before it can manifest itself by the symptoms which accompany its development. There is a little village, or rather a collection of a few houses, in my late dispensary district, called Killeen's Cross. There may be in or about fifty people residing in these cabins.

I got not one single case of small-pox in this locality during the entire epidemic, though the disease was everywhere surrounding it, and though it appeared in every other part of the district.

This is to me a very curious and unaccountable fact. I see no way of accounting for it but by the want of susceptibility on the part of these persons to take the disease. Epidemic influence was present; contagious elements were not wanting, and yet no one succumbed to their combined effects. Niemeyer says:—

"This individual predisposition has always been very unequal, for in former centuries also there were some persons who were perfectly insusceptible to variola poison, so that they could expose themselves to it with impunity; there were others in whom the susceptibility was so slight that they were only attacked by the milder forms (varioid), while in most persons it was so decided that, on exposure, they were affected with the severe form of the disease (variola)."

The next three facts which I have put forward, as proved by the past epidemic, have reference to *vaccination* and *re-vaccination*.

Firstly, the beneficial results of vaccination as shown by the statistics just read.

Secondly, that vaccination loses its preventive and protective power after a certain period of time, differing in various individuals as shown by the numbers of vaccinated who contracted the disease. This shows the value of re-vaccination, which is also proved by the statements I now read for you of those physicians who have written on this subject.

I can certify to the fact that though I saw at least 1,300 cases I knew but two instances of re-vaccinated persons having the disease. I had two cases who within the usual period of incubation were re-vaccinated and had small-pox (one of whom died, though with a large vaccine vesicle on the arm), a mother and infant were admitted to the Fever

Hospital, the mother about the sixth day of the disease. The infant was nursing and continuing to do so all through the illness.

I re-vaccinated the infant at once on seeing her (she had not been vaccinated up to this period). The mother got a severe attack, but recovered. The infant showed severe confluent small-pox, the face became covered with a universal scab, also the feet and legs. The vaccine appeared to take, as a redness came and continued to the fourth or fifth day, when any distinction was lost in the surrounding eruption. The infant recovered with the loss of an eye (through carelessness on the part of the mother). The terribly severe character of a number of the vaccinated cases is beyond a doubt. I consider this is accounted for by the fifth fact I have stated, that is, *the absence in a vast number of cases of a proper vaccine cicatrix on the arm.*

The inference is that vaccination, improper or careless, is most strongly to be condemned, and that it would be a wise step on the part of Government to appoint vaccination inspectors, whose sole duty it should be to see to the successful results of the operation, as shown by well-marked cicatrices. I do not mean to convey that cases in which the operation appeared to have been well and efficiently performed, did not take the disease and die; on the contrary, some of the worst cases of small-pox I saw during the epidemic were those in whom the operation appeared to have been most carefully performed, but they were marked exceptions, and by no means numerous, while, on the other hand, I saw many extremely severe cases amongst those who had imperfect and doubtful vaccination marks. The effects which vaccination had on the course and symptoms of the disease in this epidemic appeared to me to be those described by all former observers, more especially on the secondary fever. I was particularly struck with the little influence it exerted in the prodromal stage, some of the fiercest instances of delirium and premonitory fever occurring in the vaccinated.

Its malignancy.—I think there can be little doubt that the type of the epidemic was a malignant one as evinced by the mortality.

This was more marked outside, where the patients, being kept at home, under the most unfavourable circumstances (especially children), had the confluent and in many instances the purpuric form of the disease.

The Effects of Overcrowding, Bad Sewerage, and Dirt in Spreading the Disease and increasing the Mortality.—The disease spread in this city, chiefly from the unwillingness on the part of patients to go, and friends to send, parties to the different Hospitals, in fact every dispensary physician was aware how impossible it was, in many instances, to get even starving paupers to avail of the hospital; but in addition, the disease lighted on many localities under the most favourable circumstances for its dissemination, filth, overcrowding, defective sewerage. Certainly one-third of

the cases in my district, and a large proportion of the deaths, occurred in a small circle, having its centre at the top of the old market-place, and including in its area the two Burnt Lanes, Collins'-lane, part of Fair-lane, Harding's-lane, and other lanes off Corbett's-lane, and portion of Blarney-street.

The report furnished by me to the Corporation, an extract from which I read for you, shows what the conditions of these lanes were, and is a positive proof of the consequences of defective hygienic precautions as regards the spread and mortality of small-pox.

This report was written on the 14th September, 1871, three months before the outbreak of the epidemic:—

“I must comment on the fearfully over-crowded condition of parts of my district, the lower part of the Burnt Lanes, Nos. 1 and 2, are dens of filth, over-crowding and wretchedness, and also in Cattle-lane, and the left-hand side of Fair-lane, the houses low, ill-ventilated, and filthy, no sewerage, and deficient water supply, I respectfully submit require looking after. I need hardly allude to the effects on the morals of the people by this over-crowding, hence these two Burnt Lanes are well known for the character they sustain and the scenes that are enacted in them. It would be absurd to say anything on the character of the ventilation of houses, but one word on the lodging-houses existing in this neighbourhood may be useful. They are mostly situated in bad localities—Cattle-lane, Kerry-yard, and all about here—they are often full to overflowing, and the result is that in wretched small rooms and often lofts, patients are congregated, hence frequently it is from such that I find cases of contagion or epidemic disease show themselves.”

The remarks of Drs. Golding, O'Sullivan, Donovan, and Holmes, on the subject are worthy of notice, as bearing on another feature connected with the epidemic, on which I have already expressed myself, that is, the sanitary precautions adopted by the authorities, who received from each dispensary physician a daily report as follows:—

<i>State the</i>	<i>Name of Physician,</i>
<i>Date of Return,</i>	
1. Number of Small-pox Patients at present under Treatment in your district	
2. Number of <i>New Cases</i> seen since furnishing last Report, with Residences	
3. Number who have gone to Hospital	
4. Residences of those remaining out of Hospital	
5. Complaints of bad Sewerage (public or private), or want of cleanliness in the district	

The peculiar features of the past epidemic, as contrasted with former ones, I have already stated, do not appear to me to have been many, and I have before alluded to them. Dr. O'Connor makes some important observations on this head of the report (*vide extracts*).

I think we may define these peculiarities thus—wider spread, longer duration, greater malignancy—and some few others, the result of the more general practice of vaccination.

The complications which usually occurred were those which have been generally observed elsewhere during previous epidemics.

Those reported to me, and others which I personally observed, were as follows:—

1st. Purpuric and hæmorrhagic complications, hæmorrhage from the bowels, bladder, uterus.

2nd. Laryngeal and chest affections, including œdema of the glottis, pneumonia.

3rd. Inflammations and sloughings of the cellular tissue, sometimes leading to gangrene.

4th. Ophthalmic complications (notably few in number), as contrasted with former epidemics.

5th. Pneumonic complications.

6th. Cerebral inflammation attended with delirium; this delirium being of various kinds and degrees.

The sequelæ in order of frequency:—

1. Boils, abscesses in all parts.
2. Joint affections, the result of synovitis.
3. Chest affections, acute phthisis.
4. Erysipelas of various parts.
5. Ulcers of the cornea.

I noticed particularly the virulence of the symptoms in the *full and plethoric*. Some of the worst cases I saw during the epidemic occurred amongst the constabulary, and some of these were splendid men, with great muscular development. In fact, I believe the disease was particularly fatal and severe amongst the policemen admitted to the North Fever Hospital.

Ophthalmic Complications.—I saw altogether at the Ophthalmic Hospital from the outbreak of the epidemic 10 cases in which the eye was staphylomatous or lost after small-pox; also some cases of minor importance, but this number out of a total of about 1,000 cases attending was extremely small.

The purpuric cases which occurred did not present any unusual features as contrasted with those in the Dublin epidemic. I saw some few cases of pure malignant purpura during the epidemic, one fearful

case of which was admitted to the North Fever Hospital, a man named Kinealy, on the 10th of April; he survived about 36 hours.

He had hæmorrhage from every outlet of the body, his eyes were dark blue, the skin deep blue and purple in parts, his whole body one livid sheet; yet strange, this man's intellectual faculties were not much impaired to the last. I saw no delirium in any of those hæmorrhagic cases. They occurred when the epidemic was at its height.

The fatal day of these purpuric cases was generally the third, fourth, or fifth from the appearance of the eruption. In the cases of a severe purpuric type the papules were dark purple, and no attempt at maturation, and the eruption on the side and chest of an erysipeloid character. In a few the skin was tense and *doughy*, and covered with innumerable small pustules. I saw one case of this kind recover. There was no hæmorrhage.

The *purpuric cases*, with the characteristic spots, reminded me strongly of the analogous cases we had some few years since, when scarlatina was epidemic. It appears worthy of remark, that during the prevalence of any zymotic disease as an epidemic these purpuric cases here and there crop up.

Throat Complications.—By far the most serious complication which I had to deal with, in almost every severe case, was the laryngeal, yet I cannot recall to recollection any case which terminated fatally from this alone, though it must have accelerated the fatal termination in a great many.

I was much impressed with the advantage accruing from early treatment as regards the throat; in fact, this was one of the only satisfactory results from special treatment which I experienced. This consisted in every case in the free use of hydrochloric acid and chlorate of potash gargles, or permanganate of potash. In severe cases also the topical application of strong solutions of carbolic acid or perchloride of iron in glycerine. I cannot say that I could trace any connexion between the delirium present in a great many instances and the previous history of the patients. Some of the wildest, as also some of the most unmanageable patients, being to my own knowledge very temperate in their previous habits.

I now come to the question of treatment. I can only speak on this head from my own personal experience. I gave one remedy a fair and ample trial in this epidemic, to test its action, and also the asserted benefit of the class of remedies it belongs to (antiseptics).

I am afraid many confound, when speaking of antiseptics, the two terms disinfectant and antiseptic. Experiments have proved beyond question the property existing in certain substances of destroying vibrios and bacteria, products of vegetable and animal decomposition.

These properties are possessed in varied degree by various bodies; for example, sulphurous acid, chloride of lime, permanganate of potash, carbolic acid, &c.

Granted the truth of the germ theory of disease in such a malady as small-pox (and the power of the poisonous principle, whatever it may be, to multiply itself, or produce a given materies morbi all over the body, which results in certain pyogenic putrefactive processes); could we then find a substance which on entering the system was capable of arresting this septic process (taking that view of the word septic), or lessening the force and power of the materies morbi and the products of its dissemination in the tissues. This I would look on as an *antiseptic* in the blood, and the exhibition of remedies of this nature (in our present state of knowledge) I would call antiseptic treatment.

That the use of disinfectants is imperatively called for in all forms of zymotic disease is a fact that the merest tyro in medicine knows, and each has his favourite disinfectant, and adopts various methods of employing it.

I am not speaking now of the use of such in rooms or dwellings, but also to the surface of the body to the throat, and the several passages.

During the late epidemic I had abundant evidence of their value thus employed. The smell in the wards of the hospital was very sickening until we made a rule to fine every nurse who had not the cover of the night chair up and the vessels half filled with permanganate solution; this was put in immediately after the vessel was used, hence it was converted into a means of disinfecting the air about the wards. The effect was marvellous, the peculiar odour nearly entirely disappeared.

I take it that the value of disinfectants in the prophylaxis and treatment (externally) of zymotic disease is settled. But I looked for your opinion of your experience of antiseptics (*internally*) in the epidemic just past. The replies I received were, save in a few instances, unsatisfactory, and certainly in the majority not favourable to this line of treatment. All are present, and they can give us the benefit of their opinions and experience in person on this important matter. For my part, my treatment of nearly all the bad cases of small-pox in hospital and in private is comprised in a few words—disinfectants largely in every possible way, antiseptics internally, stimulants (chiefly brandy in the form of egg mixture). I determined at the outset to give some one antiseptic a fair trial—all through the epidemic in hospital I selected zylol. For its composition and action I refer you to the admirable paper on the use of antiseptics read at the Society of the College of Physicians by Dr. Cameron.

Save so far that the mortality in the cases in which I used the drug was low, I cannot speak much of its effects after the most careful watching.

I gave it in some 150 cases in hospital and outside; I saw some of the most malignant cases recover under its use. But some I observed without it.

I used none of it in dispensary practice, where I relied chiefly on *chlorate of potash in large doses*.

I watched carefully the cases, and I could find no material difference in any respect, save this, that I became impressed with the belief that to a certain extent it modified the force and severity of the symptoms *in or about the eleventh and twelfth days*, the period of secondary fever. I do not think that this was imagination on my part, for I several times had full opportunities of contrasting the course of the disease at the same time in patients who were and were not taking zylol.

I have no object save to give you my experience, a pretty large one, of this medicine. I gave it in 10 and 15 drop doses every hour and every second hour, with spirit of chloroform and mucilage.

I can also assert that I saw patients apparently going to the bad in the disease, and improve on the administration of this drug; others it appeared to do no good whatever to; on the contrary, on account of its unpleasant taste and effects on the stomach, I was obliged to discontinue it.

On the whole, my experience of this plan of treatment has been such, that I would, under certain circumstances, and dependent on the class of case, pursue it again; but I believe that in small-pox, as in other diseases, no fixed plan of treatment can be laid down, but that he will best guide his patient to a successful issue who, while he supports the vital powers by a careful administration of nourishment and stimulant, watching complications, administers drugs with a sparing hand, and while I look on antiseptics (so-called) in a most favourable light, I do not believe that they possess all that boasted power over this disease which some would have us believe. The treatment of the complications and sequelæ it is unnecessary for me to allude to. Any points of interest in these matters will be elicited in the subsequent discussion. I found the greatest benefit follow (in the earlier stages of the eruption) the use of the oxide of zinc and calamine lotion (3ii. of each in 3x. of rose water), laid on with a brush and allowed to dry on the face and hands, and constantly renewed. During the period of maturation and subsequently, an application of olive oil, glycerine, carbolic acid (one part in sixty), with oxide of zinc, was what I generally applied.

I have no experience of baths. Time does not permit me to enter into the question of the tent system of hospital, the benefit of which I am strongly inclined to think favourably of.

The majority of those who have reported to me have expressed themselves as favourable to it. Opinions on this point would be most important, with a view to the management of a future epidemic.

Propositions for Discussion.

1st. Small-pox is contagious.

2nd. Re-vaccination is essential after seven years of age, and should be repeated.

3rd. A tent system of hospital would be under certain circumstances worthy of a trial in the event of another epidemic.

4th. The use of antiseptics in treatment deserves close consideration, and though we have not at present sufficient data on which to form sound conclusions, yet they are such as would warrant a full trial of this class of remedies in future epidemics.

Extracts from Reports of various Hospital and Dispensary Physicians in Cork on the Small-pox Epidemic.

PROFESSOR O'CONNOR—I saw no case of small-pox in a vaccinated person under ten years of age. Sometimes after a high fever, with a rose-coloured rash on many parts of the body, particularly the wrists and back of the hand, nothing followed but a slight vesicular eruption. I saw but few cases after forty, and these were rapidly fatal, characterized by purple spots on several parts of the body and hæmorrhage from internal organs. I re-vaccinated over 200 individuals. I saw no unpleasant result in any case, owing, I believe, to the great care taken by them at the period of maturation. I did not see any one attacked by small-pox who had been re-vaccinated, though I often performed the operation in the house where the disease prevailed.

In my own family, my son, who would not get re-vaccinated, was attacked with the disease, while the re-vaccinated servants who nursed him escaped.

I witnessed two epidemics of small-pox, one in the year 1839-40, and one in 1850. In neither of these did the disease spread amongst the better classes, though re-vaccination was never practised; from this it would appear that the power of disseminating the germs of the poison was greatest in the late epidemic.

Two causes, I think, appeared to produce this result. First, many of the people in the earlier period had been inoculated, vaccination not having been extensively practised until 1810 to 1815, and the former preventive is undoubtedly the most powerful. Secondly, it appears to be a law of epidemics that when they return after a long absence they are disseminated more rapidly, and exhibit symptoms of greater virulence; and the late epidemic returned after an absence of nearly twenty years.

The second great difference which I observed between this and former epidemics was in the hæmorrhagic cases, which I never witnessed formerly. The only appearance bordering on it was that at the period of maturation a sudden collapse took place, the pustules fell in and contained a slight

bloody exudation, with perhaps some pus. My experience in this epidemic has not been large, but I was struck with the resemblance or identity of the delirium with that of delirium tremens, which made some think that it proceeded from the previous habits of the patient.

DR. CALLAGHAN, Physician to the Workhouse Hospital—I have never seen a case of small-pox after re-vaccination, nor have I seen any unpleasant results from it.

I do not believe that antiseptics produced any effect.

DR. DONOVAN, Physician, City Dispensary—The disease was in many cases ushered in with smart febrile symptoms, followed in some instances with what may be termed a scarlatina rash, which faded away in a short time, then came the papula of small-pox, very few indeed, no maturation, and the disease, as it were, exploded and passed away immediately.

The usual complications were sore throat, and in a few cases ulcerations of the cornea, purpuric cases, with bleeding from the bowels, kidneys, and lungs.

In a great many instances, after the disease had disappeared, I had very severe deep-seated inflammation of the cellular tissue with abscess, and ugly boils in various parts of the body.

I have not used antiseptics internally, but I found carbolic acid used externally a very valuable adjunct, in correcting the bad odour, and allaying the unpleasant itching of the skin. Nearly three hundred cases occurred in my district. The first case was seen in December; the number of deaths registered were 13.

Overcrowding and bad sewerage most certainly helped the mortality and spread of the disease. It was worthy of notice that in Sundays Well, which is a portion of my district, very little small-pox appeared; this might be mainly, I would suggest, attributed to the proper sewerage, and not overcrowding of the houses, as compared with other portions of the district.

During the epidemic I could nearly always guess the different houses in the district that the disease would appear in.

I am of opinion that the sanitary precautions adopted by the authorities were not instrumental in checking the progress of small-pox in my district.

DR. CUMMINS, Assistant-Physician, County Infirmary—As a general rule, my experience leads me to believe that vaccination confers protection against an attack for about six years, and modifies the disease considerably for a much longer period, and sometimes for life. There are, however, many exceptions to this rule. About twelve months prior to the last epidemic, I re-vaccinated great numbers of my patients, and after

the epidemic appeared many more, amounting altogether to several hundreds, from six years old and upwards.

Some of these cases had had small-pox, and were pitted deeply, and yet took the vaccination more or less perfectly.

In my experience, re-vaccination confers complete immunity from the epidemic, during, or immediately before which it is performed. All my re-vaccinated cases escaped entirely, though some of them were often exposed to the contagion in its most virulent form, except one, where there was a doubtful history, of a mild attack.

I have only seen an unpleasant result of vaccination in one instance, and it was doubtful whether it was due to the re-vaccination, as the arm had healed up entirely, and all inflammation had subsided some weeks before the outset of the disease. The patient (a lady) had a most serious pectoral abscess, which ultimately extended into the loose areolar tissue of the axilla, and threatened her life for several weeks.

The past epidemic has confirmed my previous opinion that, although small-pox is contagious, contagion alone is unequal to causing a general spread of the disease when *epidemic* influence is wanting. Small-pox prevailed for a long time in England before it spread to Ireland, and was afterwards for months in Dublin before it spread to Cork, although the first communication existed between the infected and non-infected localities, and isolated cases were frequently imported to the latter, but failed to cause any general spread until the epidemic influence so long delayed had arrived.

The most striking feature of the late epidemic was, that, notwithstanding its intensity and great fatality, some of the worst cases were so little pitted, and so few lost their sight.

DR. GOLDING, Physician, City Dispensary—In a square in my district there are about 150 inhabitants. A girl in one of the families had small-pox early in the epidemic. Accommodation could not be had for her in the Fever Hospital until such a stage of the disease that the people elected to keep her at home. I re-vaccinated the father of this girl and another tradesman, who did not believe in it, just one fortnight from the first day of the rash on the girl. The father took sick of the small-pox; he went to the Fever Hospital, and died. His fellow-tradesman put him into the coffin, and exactly in twelve days he showed symptoms of small-pox, with the rash in three days after, but being a strong man, he, after a very severe attack, recovered.

I take this to be not only a strong instance of its contagious nature, but a strong proof of the value of re-vaccination, as not another person in the square then or since took the disease.

DR. O'SULLIVAN, Physician, City Dispensary—I consider re-vaccination, when performed in proper time, a preventive of small-pox.

In a large institution, all the inmates of which had been re-vaccinated, a stranger was admitted. Symptoms of small-pox showed themselves in this person a few days after admission. The patient having been removed to the Small-pox Hospital, none of the other inmates, although some of them had been sleeping quite close to the infected patient, were attacked by the disease.

As a further instance, I may state that I have not been called to attend a case of small-pox in any person who has been re-vaccinated at a time prior to having been exposed to the contagion of small-pox equal to the ordinary period of incubation; but I have vaccinated and re-vaccinated persons within this period, who, having been previously exposed to the contagion, took the small-pox, and in some of the cases the vaccine vesicle went on simultaneously with the eruption of small-pox.

I had no accident worth recording from re-vaccination. I observed the disease to take apparently a definite direction, from east to west. As to its contagious nature, it may be inferred from the fact that where proper sanitary measures and sufficient isolation of cases was not enforced, the numbers struck down in particular localities were in direct proportion to the neglect of such precautions.

I am of opinion that overcrowding and bad sewerage in a district tend to the propagation of small-pox, and thereby to increase the mortality from this disease.

I have found the mortality greatest in such localities.

I may relate that in a largely inhabited, but well-constructed establishment, a single case of small-pox occurred as if sporadically, although I have no doubt that the contagion must have been introduced in some latent manner, probably in soiled clothing. Precautions were taken to prevent the spread of the disease—1st. By the isolation of the patient. 2nd. By disinfecting the room, surrounding the corridors, and neighbouring rooms by means of carbolic acid used several times daily in the form of spray and otherwise. None of the inmates were re-vaccinated, and no other person in the establishment contracted small-pox.

The absence of fear of contagion amongst the poor I believe had much to do with the spread of the disease. I invariably found strangers and neighbours in and out of the houses of the infected.

The only measures of any advantage taken by the sanitary authorities worth mentioning were the washing of the lanes and occasionally destroying the clothes and bedding of the sick.

As for the disinfection, it was quite worthless. It consisted mainly in sprinkling a solution of chloralum about the houses, the quantity used being absurdly small when compared to the disinfection required, and being confined to the floor as a matter of course, could have no influence in an atmosphere charged with organic debris, containing the materies
1 bodies of the sick.

DR. M'EVERS, Physician, Cork Fever Hospital—I have had no instance in my practice of a re-vaccinated person having contracted small-pox.

I have had a few cases of deep-seated inflammation affecting the re-vaccinated, in some cases also attended with inflammation of the axillary glands, which subsided under ordinary treatment. I have no doubt about the contagious nature of small-pox, but I was forcibly struck with the fact that the only persons in attendance on small-pox patients in the Cork Fever Hospital during the epidemic who contracted the disease were the Protestant chaplain and a medical student (my own son). All the nurses escaped, although some of them had not been previously vaccinated.

I have no faith in antiseptics in the treatment of small-pox.

DR. CREMEN, Physician, Cork City Dispensary—My experience of re-vaccination has been as follows:—I re-vaccinated at least 900 persons. Out of that number only one solitary case of small-pox, to my knowledge, has occurred after re-vaccination, and in this instance the poison of small-pox must have been in the system prior to vaccination. He sickened on the next day after vaccination. The disease, which was of a mild kind, took the usual course.

As a proof of the preventive effects of re-vaccination I may mention the instance of the Government Orphanage and Industrial Schools, of which I happen to be medical attendant. In this establishment there are about 140 boys (intern). At the very commencement of the epidemic we had one case of small-pox, which on seeing I ordered at once to the Fever Hospital, and had bedding and dormitory disinfected. On the two ensuing days I procured a supply of lymph to vaccinate the entire school, and did so.

Not one of those boys were seized with small-pox, although it surrounded the school on all sides. I have not seen any unpleasant results after re-vaccination.

DR. GRATTAN—I have care of about 300 children in different schools. At the outbreak of the epidemic all these children over seven years were re-vaccinated, and all others who had not two or three marks of vaccination upon their arms; but one of these children took small-pox, and that in so mild a form that the disease was scarcely recognizable. This child was not more than seven days ill.

I may add that on four occasions small-pox was introduced into these schools by children, in whom the disease developed itself in two or three days after their admission into the school, and who had not been re-vaccinated.

DR. TOWNSEND, Junior, Physician, South Infirmary—I have not seen a single case of the disease amongst persons recently re-vaccinated.

although I re-vaccinated many persons where one member of the family had already the disease.

Not a single person so re-vaccinated took it, and in one case a girl whose arm was sore slept in the bed with the small-pox patient, yet she did not take the disease.

I have seen no bad results from re-vaccination except an occasional sore arms.

DR. PORHAM, Physician, Cork Workhouse Hospital—I consider re-vaccination to operate strongly as a preventive. I cannot otherwise explain the almost sudden collapse of the late epidemic when re-vaccination had been actively carried out.

I have seen some injurious effects in tubercular habits and constitutions which would make me to hesitate about re-vaccinating in incipient phthisis or those threatened with it.

I have occasionally seen some troublesome sequences from it, such as ecthyma, impetigo, farunculus, and in one case a deep abscess under the pectoral muscle and pyæmia which nearly proved fatal.

The average cases pass off favourably. One remarkable difference was the less frequency of ophthalmic cases in the late epidemic.

I am disposed to think favourably of antiseptics, but not having tried some of them till late in the epidemic I have not experience enough to speak with certainty. I have a good opinion of chlorate of potash in full doses.

DR. W. C. TOWNSEND, Physician, South Infirmary—1. Considers antiseptics useless. 2. No bad result from re-vaccination.

DR. HOLMES, Physician, City Dispensary—Overcrowding and bad sewerage increased the mortality in my district to a great extent. Sanitary precautions, an elaborate farce, did not do one iota of good.

HENRY EAMES, M.D., &c.,

Physician to Mercer's Hospital; Lecturer on Practice of Medicine in the
Lewish School of Anatomy and Surgery; Hon. Secretary to the
Medical Society of the College of Physicians.

DIED MARCH 24, 1873.

It is not usual to record in this Journal the deaths which occur in our ranks. We have not noticed any of those which have taken place since (in 1869) Maurice Collis fell a victim to his professional ardour; but we cannot resist the promptings of personal regard in the case of HENRY EAMES, who, a few days ago strong and well among us, has now entered into his rest, stricken down by disease contracted in the exercise of his profession.

Although raised by circumstances above the necessity for exertion, courted by many friends on account of his unfailing store of bright and genial wit and surrounded in the closer circle of home by all that could make life enjoyable, he yet devoted himself with rare assiduity to the study and the practice of medicine, and died of that dreadful typhus fever which each year claims some life, and many years claims many a life among those who do battle against it in this city.

DR. EAMES was highly distinguished during his University career, and took several honours in classics. Almost immediately after his graduation he obtained the appointment of Physician to Mercer's Hospital, where he worked diligently, both as a Learner and a Teacher, and as the just reward of his exertions, he was steadily rising in public estimation when, at the early age of thirty-one, he was called away, leaving to not a few among us the feeling of personal bereavement.

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MEDICAL SCIENCE.

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PART I.

ORIGINAL COMMUNICATIONS.

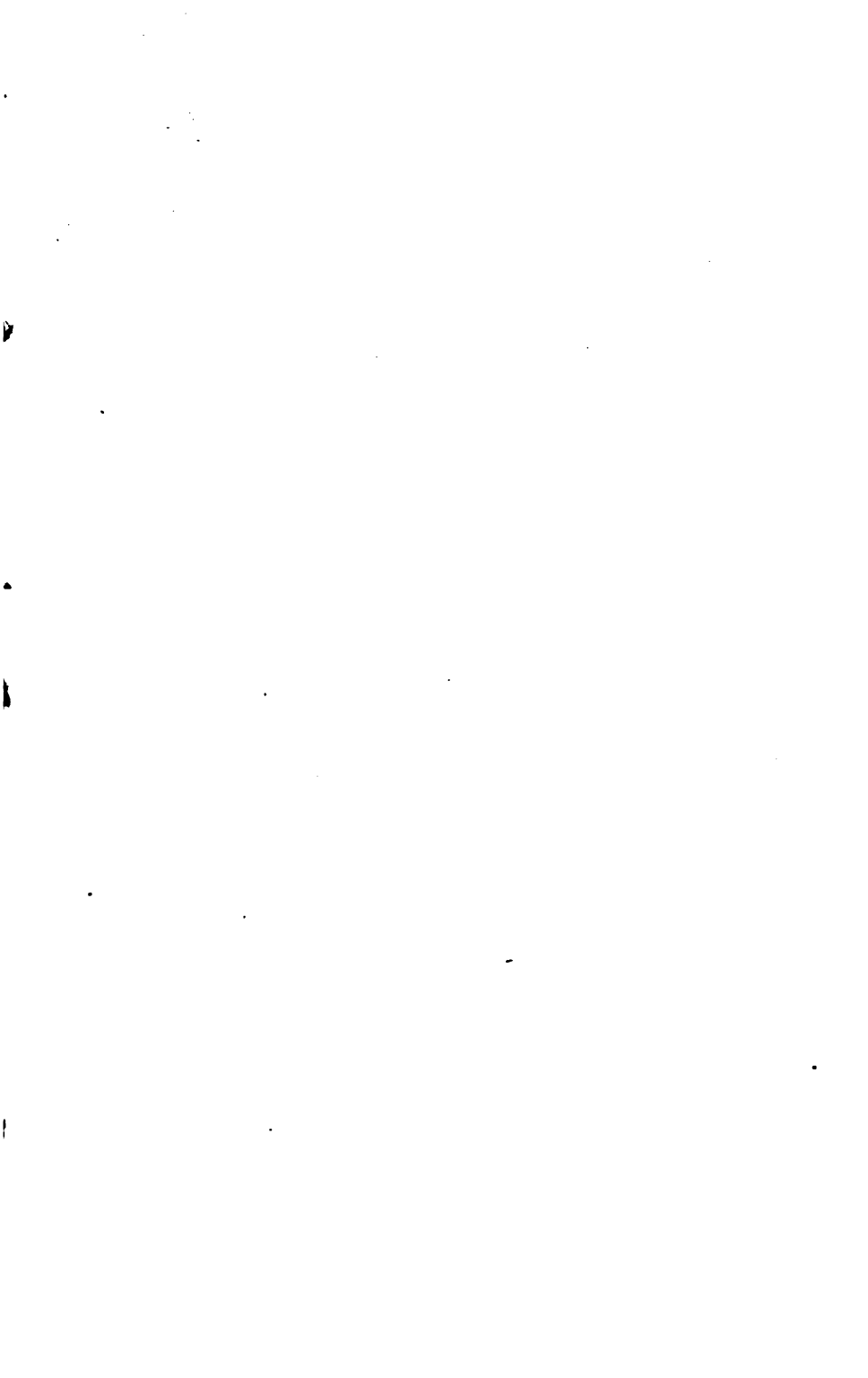
ART. IX.—*Observations on Diffuse Inflammation of the Areolar Tissue of the Neck (Cellulitis): Importance of early, free, and deep Incisions; with practical reference to the Surgical Anatomy of that Region.* [Illustrated by Cases and Diagrams.] By HENRY GRAY CROLY, F.R.C.S.I., Surgeon to the City of Dublin Hospital; Lecturer on Clinical Surgery; Senior Demonstrator of Surgical and Descriptive Anatomy, Royal College of Surgeons; Member of the Council of the Surgical Society of Ireland, &c., &c.

DIFFUSE inflammation of the areolar tissue of the neck (cellulitis) is one of the most serious and fatal forms of disease which the erysipelatous type assumes. Several urgent cases of this nature having been under my care, in my hospital and private practice, during the last few years, I feel considerable interest in the subject, and deem it of sufficient moment to claim the particular notice of the profession; and, considering the vital importance of the various structures implicated in deep-seated inflammation of the neck, viz., the contents of the sub-maxillary region, the base of the tongue, the larynx and trachea, œsophagus, great vessels and nerves, the affection has not, in my judgment, received that amount of *special* attention by surgical writers which I consider it imperatively demands. The symptoms are urgent and alarming, and if relief be not afforded by prompt and bold surgical aid, the patient may be suffocated; and even under the best surgical skill

(as in the instance of a late revered and distinguished member of the profession), the complication of blood-poisoning and its fatal effects unhappily frustrate the best-directed efforts of the surgeon. This serious affection occurs as the result of exposure to wet and cold, or from some obscure and constitutional cause, such as occasions erysipelas and other zymotic diseases at special seasons. The attack is ushered in by rigors, lassitude, headache, thirst, and other symptoms of fever, pain in the neck and loss of sleep. The parts engaged soon become swollen and tense, conveying to the touch the sensation termed "brawny," indicating infiltration of fluids into the under-lying areolar tissue and inter-muscular spaces. There is pitting on pressure, especially in the mesial line, over the larynx and trachea (*without fluctuation*); dyspnœa and dysphagia next become urgent symptoms, increasing much the sufferings of the patient, and foreboding danger. The breathing is stridulous, the voice whispering, and the countenance assumes a very anxious expression, the forehead being wet from drops of cold sweat. The patient feels much exhausted, dreads choking, and (as I have witnessed in some instances), from being unable for several days to swallow solid food, or even fluids (the latter regurgitating through the nose), soon loses strength, and danger to life becomes imminent. The platysma muscle, in its cellular sheath, with the layers and reflections of the cervical fascia, bind down, with much tension and mechanical resistance, the subjacent infiltrated and swollen tissues, and consequently exert painful compression on the root of the tongue, the larynx and trachea, œsophagus, vessels, and nerves.

The following series of cases, which were under my care, will illustrate this practical subject, exhibiting the various forms which inflammatory affections of the neck assume, with the treatment and results :—

CASE I.—Patrick Farrell, aged twenty-seven, consulted me on the 28th of last January for a swelling of the neck. He had been temperate and healthy; noticed a pimple over the larynx some days previously; had rigors on the 25th and 26th, and felt some difficulty in swallowing. On examination I found fulness under the chin, reaching from angle to angle of the jaw, somewhat in appearance like a muff round the throat. The left sterno-mastoid muscle was more prominent than the right; a hard brawny swelling extended over the larynx and trachea to within about an





Forster & Co. Lith. Gr. St. Dublin

MR CROLY ON CELLULITIS OF THE NECK

inch of the sternum. The symptoms were dysphagia, dyspnoea, cough with a ringing sound, and accompanying high fever. Purgatives were ordered, and the constant application of linseed poultices.

January 30.—The patient had disturbed sleep. Redness over the larynx and trachea.

January 31.—Appearance of the neck drawn by Herr Tomsohn, of Messrs. Forster (*vide* lithograph, Plate XVI).

February 1.—Passed a very restless night, with raving. When attempting to swallow, the fluid regurgitates through the nose; eight days without solid food; dyspnoea more urgent; the swelling much increased, and pitting on pressure.

The patient's head being held steady by an assistant, I made a vertical incision with a scalpel over the crico-thyroid membrane, about an inch in length (opening the cervical fascia on a director), and a second deep vertical incision between the chin and os hyoides. The structures thus divided were dense, brawny, and infiltrated; blood and serum only escaped. The patient experienced *instantaneous relief*, and was soon able to swallow with comparative ease. Beef-tea was ordered, and linseed poultices continued.

February 2.—Redness and swelling much diminished.

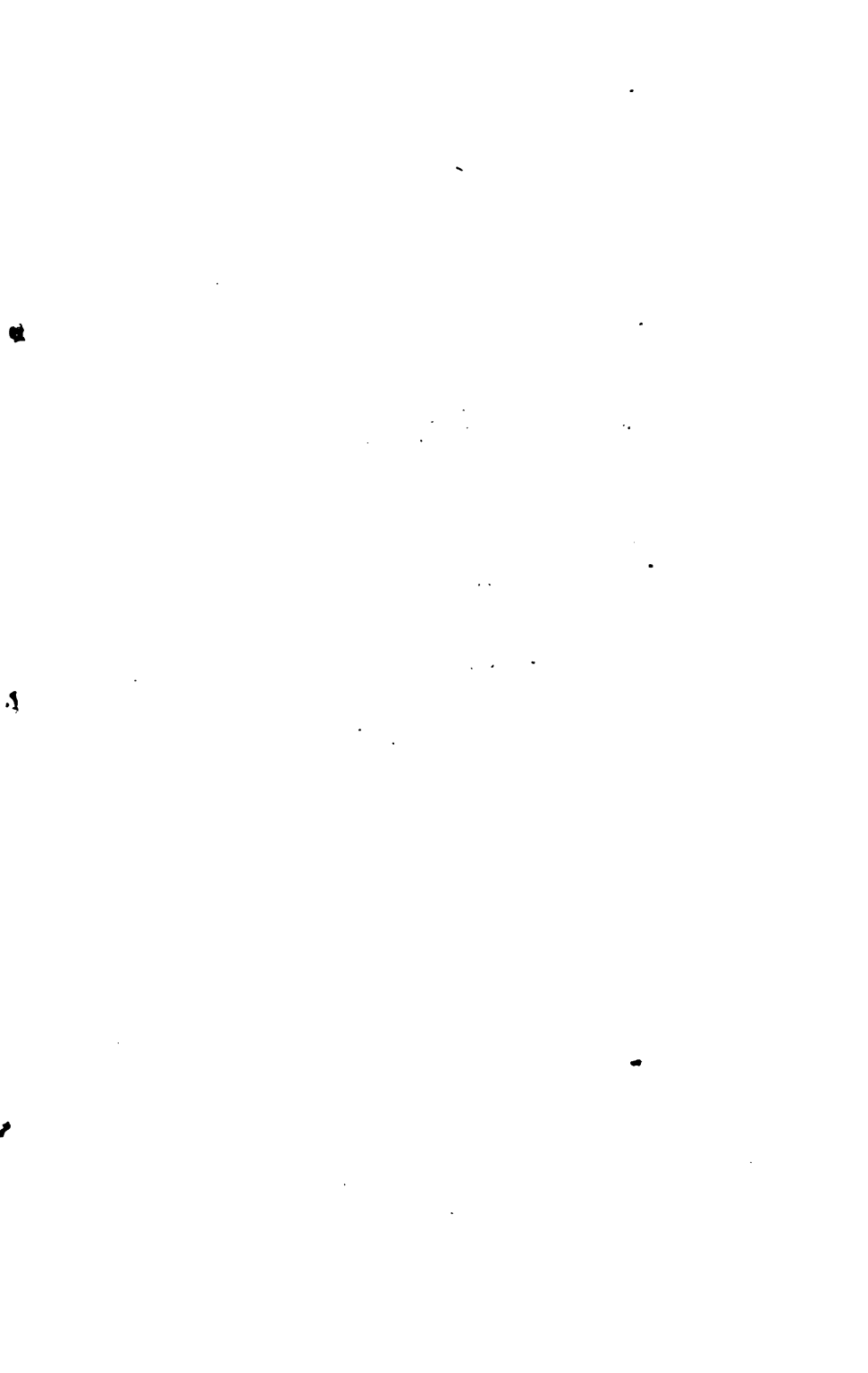
February 3.—A circumscribed abscess appeared (over the os hyoides), which I opened, giving exit to healthy pus. Ordered decoction of cinchona bark, with chlorate of potass. The patient made an uninterrupted recovery.

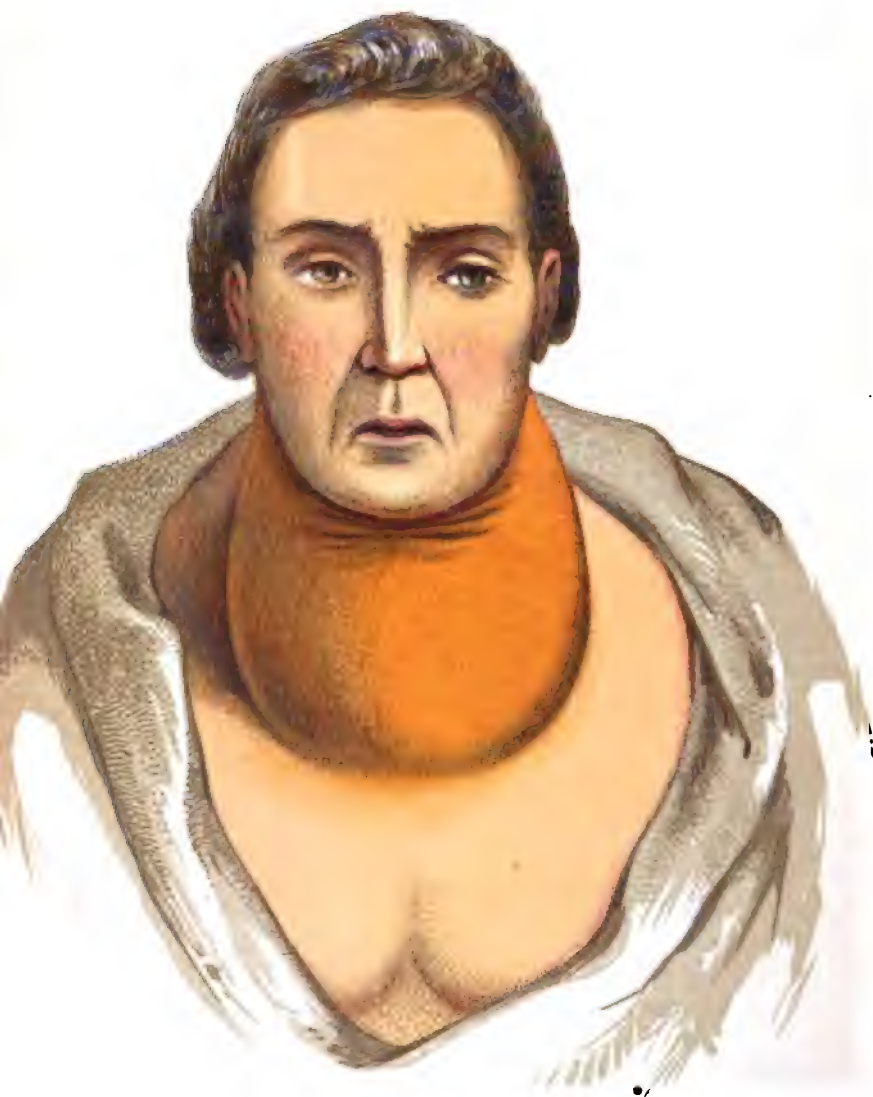
CASE II.—I was sent for to the country on the evening of the 16th November, 1872, to see Mr. D——, aged twenty-eight, in consultation with Dr. Jones, of Rathgar. I was informed that the patient had been previously strong and healthy, and was “now choking.” On arriving at the house I found him in bed, partly in a sitting posture; his breathing stridulous, pulse rapid, and cold sweat on the forehead. He could scarcely speak. I learned that about three weeks previously he got a severe wetting, after which he was seized with pain in the ear. Redness and swelling of the neck were next observed. His medical attendant directed leeches, fomentations, and poultices. On examination I observed the right sterno-mastoid muscle very prominent, and a dusky red swelling (over the trachea) which pitted on pressure. There was *not any evidence of fluctuation*. The patient had not swallowed food for several days. I informed him and his friends of the urgent necessity for

prompt and active treatment. Accordingly I proceeded to operate, assisted by Dr. Jones. I made an incision about an inch long through the integuments, along the posterior border of the sternomastoid muscle, at about the junction of its middle and lower third, and next divided the fascia on a director to the same extent; bubbles of gas escaped, with turbid, serous fluid. *Instantaneous relief* was felt; the patient spoke clearly, and swallowed without difficulty. I directed strong beef-tea, eggs beaten up with brandy, and hot linseed poultices. At my next visit, lint with carbolic oil was inserted into the incision. Decoction of cinchona bark with chlorate of potass, as in the previous case, was prescribed. The patient quickly recovered.

CASE III.—Thomas Gray, aged sixty-six, was attacked in Feb., 1871, with rigors, pain in the left side of the neck, accompanied by high fever. His respiration and deglutition were interfered with. He had no sleep, and passed through a week of suffering without having been able to swallow solid food. When I saw him the left sternomastoid muscle was prominent and tense; dyspnoea was urgent, and the expression of countenance very anxious. From the history and symptoms I was satisfied as to the nature of the case, and of the necessity of relieving tension, by freely opening the cervical fascia. I made an incision, as in the last case, with a scalpel along the outer edge of the mastoid muscle, and, with Syme's knife, divided the fascia; blood and serous fluid escaped. The patient experienced *immediate relief*. Poultices, nourishment, and tonics were ordered, as in the previous cases, and the patient rapidly recovered.

CASE IV.—Anne Delany, residing in Cork-street, aged twenty-eight, married, mother of three children, was admitted into the City of Dublin Hospital, April 24, 1868. Enjoyed good health previously; had goitre for some years at the left side of the neck, not causing any annoyance. Was confined of her last child on the 5th inst. Two days after her confinement she had a cough, and was feverish. These symptoms continued until the 30th, when she had a rigor. She vomited, and felt more swollen in the neck at the goitre. The swelling increased rapidly until the day of her admission to hospital. On examination I found an *enormous enlargement*, of a peculiar dusky colour, occupying the entire front and sides of the neck, from the sternum to the chin, which were nearly on a level





(*vide* Plate XIV.). At first view the case looked like a huge goitre which had been painted with tincture of iodine. The swelling extended laterally to the angles of the jaw, and backwards towards the spine. There was considerable dyspnœa, rapid and feeble pulse, and difficulty of swallowing. She was ordered beef-tea and wine, and large linseed poultices.

April 25th.—Symptoms increased in severity; the patient was propped up in bed; pulse 132; eyes protruding; conjunctiva yellow; voice husky; pitting on pressure over the trachea. I made an incision with a scalpel over the part, and opened the fascia to relieve tension, laying bare the rings of the trachea. I then made a second incision along the outer border of the sternomastoid muscle. Poultices were applied. At first there was not any discharge from the incisions, except of blood and serum. The subsequent discharge resembled tow steeped in pus, as in anthrax and phlegmonoid erysipelas.

26th.—Patient much worse; pulse 140; orthopnœa and aphonia. Ordered a stimulating enema. These symptoms continued until the 3rd of May, when the face became congested, the respirations rapid, the hands twitching; *pain complained of in the left knee-joint*, which, on examination, was found enlarged, measuring three-quarters of an inch in circumference more than the other knee. The rigors returned, the patient sank rapidly and died.

CASE V.—Mrs. Hill, aged thirty, presented herself at the hospital, and was seen by my colleague, Dr. H. Benson, when prescribing for extern medical cases. On examination he found a goitre at the right side of the neck, with symptoms of surrounding inflammation. He admitted the woman into hospital, to be placed under my care in the surgical wards. I found that the bronchocele had existed for several years. The local inflammatory symptoms increased; the redness and swelling extended along the trachea; an incision was made over the part in the mesial line, and gave exit to healthy pus.

CASE VI.—Jane Dunne, aged fifty, consulted me, March 3rd, 1873. On examination I observed a dusky erysipelatous inflammation, extending from the upper end of the sternum to the os hyoides, and laterally to the outer border of each sternomastoid muscle. The swelling completely filled the hollow over the trachea. She had dysphagia, but no dyspnœa. No cause could

be assigned for the affection, which commenced on the 27th of February, by rigors and soreness along the throat, and a feeling of tightness. There was *no fluctuation*. Poultices were applied. A free incision was made in the mesial line over the trachea, and the fascia divided. The patient felt *immediate relief*. Tonics were prescribed. Recovery was rapid, but considerable hardness over the trachea remained for several days (*vide* Plate XV., Fig. 2).

CASE VII.—March 18th, 1873, Catherine Masterson, a child, aged a year and five months, was brought to me for advice. A large abscess had formed under the angle of the jaw at the left side, filling the digastric space (*vide* Plate XV., Fig. 1). Ten days previously the mother first observed the swelling under the jaw. No cause was assigned. It was probably connected with dentition. I opened the abscess by a *transverse* incision under the jaw through the integuments only. The fascia was scraped with the point of a bent director, and then freely divided. A copious discharge of pus followed. Brandy and milk and chicken-broth were ordered.

The preceding cases of inflammation of the tissues of the neck are of much practical importance, each presenting some feature of interest.

In the *first case* the inflammation attacked the structures beneath the chin and angles of the lower jaw, causing a swelling resembling a muff round the throat. Dysphagia was the most prominent symptom, the structures at the base of the tongue and the pharynx being implicated, with consequent risk of œdema of the glottis. There was no redness of the integuments, but a characteristic brawny condition, indicating infiltration of the parts affected. The patient was much exhausted from want of food and drink. The deep vertical incision under the chin, with a scalpel (the blade passed up almost to the handle, a distance of about two inches), and the second vertical incision over the crico-thyroid membrane opening the fascia, relieved tension, and was followed by immediate relief, the patient having been able to swallow with ease, which had been impossible for several days previously.

The *second case* was very urgent, characterized by stridulous breathing, orthopnoea, dysphagia, and cold sweat. The relief afforded by opening the cervical fascia, behind the sterno-mastoid muscle, was instantaneous, and rescued the patient from impending suffocation, as rapidly as though I had performed bronchotomy.

Fig 1.



Fig 2.



Forster & Co. Lith. from Dr. Dobson.

M^r CROLY ON CELLULITIS OF THE NECK



The *third case* was similar to the preceding in most particulars as regard the situation of the parts engaged and the immediate relief afforded by incisions.

The *fourth* was the most remarkable of the above series of cases, inflammation having attacked a goitre, so very unusual an occurrence, and having followed so soon after parturition. The intensity of the inflammation, the huge size which the neck acquired, and the very peculiar discolouration (each so well delineated in the accompanying lithograph), the urgent dyspnoea and dysphagia, the rigors, low fever, very rapid pulse, pain and swelling of the knee-joint—all pointed to intense blood-poisoning, and consequently the utter hopelessness of the case. The incisions afforded but temporary relief by opening the cervical fascia, and thus relieving tension.

A case of diffuse inflammation of the neck, in several particulars resembling the fourth, was sent into the hospital about a year and a half ago by my brother, Dr. Richard Croly, L.R.C.S.I. As I was not on clinical duty at the time, the patient was admitted under the care of my colleague, Dr. Stoker. The subject was a young man; the neck was enormously swollen; the symptoms were very urgent. Free incisions were made, and notwithstanding the best efforts to save his life, the patient sank rapidly from blood-poisoning.

The late eminent Surgeon, William Henry Porter, in his classic work on the "Surgical Pathology of the Larynx and Trachea," referring to these cases of diffuse inflammation of the neck, says:—"Operation of any kind seems to be nearly valueless, and death to be almost inevitable. Deep and free incisions appear to be the only rational mode of treatment; but we should not entertain too favourable expectations from them."

The *fifth case* was another instance of a goitre being attacked by inflammation, and extending to the deep tissues of the neck. The other features did not present any peculiarity. I saw this patient while writing these remarks. She informed me that, after her confinement lately, the goitre became inflamed, but no abscess formed.

In the *sixth case*, the inflammation was of a dusky erysipelatous hue (as seen by the coloured lithograph, Plate XV., Fig. 2). It occupied the space over the trachea, and was not so deep-seated as in the other cases, thus accounting for less urgent dyspnoea and dysphagia. The discharge, moreover, was of a healthier character.

The *seventh case* was an example of abscess occupying the digas-

tric space in a very young child. I have met with numerous instances of this kind in children during the period of first dentition. The little patients suffer greatly from pain and want of sleep, and by refusing food become much debilitated. A small "kernel" near the angle of the jaw is the first local symptom observed by the mother. No treatment prevents the formation of matter, but fomentations and poultices favour suppuration. The abscess should be opened *transversely*, by a small incision below the jaw, and the cervical fascia divided on a director. When the swelling subsides the small cicatrix will not be visible, as it recedes under the jaw. The natural objection to the knife, on the part of the mother, is often a cause of prolonged suffering to the patient at this tender age.

The recital of the history, symptoms, and treatment of the above cases of diffuse inflammation of the areolar tissue of the neck, or cellulitis, and cases of acute abscess, I consider would be deficient were I not to refer to a form of the disease, which is but too familiar to every practitioner who has had opportunities of attending cases of malignant scarlatina. I allude to unhealthy inflammation under the lower jaw, graphically described many years ago, by the late Dr. Osbrey, of Dublin (whose death, I regret to say, occurred recently, while I was writing this paper), and quoted by Graves in his "*Lectures on Clinical Medicine.*"

In illustration, I may cite the two following cases, from amongst my notes of numerous examples of this formidable affection, which have been under my own observation:—

J. R., aged three and a half years, during the third week of malignant scarlatina, was attacked with diffuse inflammation of the neck, of a dusky red colour, extending from ear to ear. The integuments sloughed, convulsions followed, and the child died. The treatment adopted was poulticing, strong beef-tea, and a mixture of chlorate of potass, muriatic acid, and decoction of cinchona bark—carbolic acid dressing to the neck, to prevent fœtor.

M. R., aged one year and a half, brother of the preceding patient, had also malignant scarlatina, and was attacked with a similar form of inflammation of the neck. The integuments sloughed under the jaw at the left side, laying exposed the parts contained in the digastric space; purpuric spots appeared over the surface of the body, and the child died in convulsions.

My own experience of these fatal cases so closely corresponds

with Dr. Osbrey's that I am induced to quote his words:—"The cases of malignant scarlatina, which usually proved fatal, were those affected with diffuse inflammation of the neck, generally in children under four years of age. The progress of the inflammation was insidious, commencing as an indurated swelling behind the angle of the jaw, often extending to the opposite side; the skin of a dusky red colour, sloughs frequently formed, commencing in dark purple specks on the surface of the swelling; the sloughing rapidly spread; diarrhœa then set in; the abdomen became tympanitic; spots of purpura at times appearing over the body; the child died either comatose, or exhausted by diarrhœa. In some cases there was sudden hæmorrhage from the large vessels of the neck. Convulsions sometimes preceded death. When abscesses form and healthy pus is discharged, the patient may recover." Dr. Osbrey further observes:—"I have in some cases made incisions into the swellings, extending beneath the fascia of the neck; but I do not consider it good practice in the cases of young children, however useful it may be in that of adults, for the following reasons:—When made in the early stage of the inflammation, they have a tendency to prevent the occurrence of either of those most favourable results—resolution or abscess; and when made in the advanced stages, unless where abscess or diffuse suppuration takes place, they are of no use; they do not appear to check the sloughing of the integuments. The parents, moreover, to whose feelings such practice is generally repugnant, are very apt to attribute whatever ill afterwards befalls their child to those incisions having been made, a consideration which I think should weigh with us in determining against the practice, when no good is likely to arise from it. In case, however, abscess, or diffuse suppuration of the cellular membrane occur, we should not hesitate to make free openings."

In the detail of the cases of diffuse inflammation of the cellular tissue of the neck, and cases of acute abscess, above recited, I have made repeated reference to the cervical fascia, as binding down the muscles and vessels of this region, and (owing to its dense and unyielding structure) to infiltration, beneath or between its several layers, causing tension, and consequent painful pressure on the trachea, œsophagus, great vessels, and nerves; thus accounting for dyspnoea, dysphagia, and the other serious symptoms above described.

It is necessary for the surgeon to bear in mind, in relation to

the Surgery of the Neck, the important part which this fascia performs in the progress of disease, and with reference to operative interference. It consists, as is well known, of superficial and deep layers, forming a kind of linea alba in the mesial line, and passing backwards towards the spine, with a separate investment to every part. The superficial layer connects and supports the platysma muscle, and is expanded on the anterior and lateral parts of the neck, reaching as high as the parotid gland. The deep layer is connected with the superficial, and passes behind the sternomastoid muscle, enclosing, as if in a sheath, the other muscles of the neck, covering the trachea and sheath of the carotid vessels, and is attached to the sternum and clavicles. This fascia, and its importance, both in health and disease, has been long since accurately described by Allan Burns, in his "Surgical Anatomy of the Head and Neck."

In making the necessary incisions in the neck, to divide the cervical fascia, and thus to relieve tension and give exit to the infiltrated fluids (as is necessary in cases such as I have above detailed), there are certain parts of the neck which can be cut into with perfect safety, while others must be avoided. In order to make this plain, I have delineated in the accompanying lithographs, "*lines of safety*" and "*lines of danger*," hoping that such a guide may prove useful to young practitioners who may be called upon, in emergency cases, to make bold and deep incisions in the neck (*vide* Plate XVII., Figs. 1 and 2).

I have not even alluded to Bronchotomy in the treatment of these cases, as I consider that opening the windpipe is not only unnecessary but dangerous; any benefit to be expected from such operation is derived from freely opening the cervical fascia, while the entrance of poisonous fluids into the larynx and trachea is thereby avoided.



Since writing the above, my attention has been directed by my friend, Dr. Nihil, Surgeon of H.M. ship "Vanguard," stationed in Kingstown harbour, to a lecture, entitled "Clinical Observations on Submaxillary Cellulitis," by Mr. Bickersteth, Honorary Surgeon to the Liverpool Royal Infirmary, published in the Liverpool Medical and Surgical Reports, Vol. iii., for October, 1869. Having communicated with Mr. Bickersteth, he kindly favoured me with a copy of his pamphlet, which I consider is a very valuable contribution to the Surgery of the Neck. His experience of the symptoms and treatment of this dangerous form of disease so



Fig. 1.



Fig. 2.

 "Lines of Safety," indicating Safety in making Incisions
 "Lines of Danger," indicating Danger in making Incisions
Forster & Co. Lith. Opp. St. Dublin

MR CROLY ON CELLULITIS OF THE NECK.

exactly coincides with my own, that I am glad to supplement what I have written by the following extracts from his able and instructive lecture:—

“The disease has a close analogy to erysipelas, and may terminate in resolution, but generally passes into gangrene. Symptoms of pyæmia often set in. When the inflammation is situated in the submaxillary region there is danger, even early in the disease, of œdema glottidis.”

Mr. Bickersteth had not seen the disease till 1861. He records six cases. The first occurred in a man aged forty, who presented himself at the Liverpool Royal Infirmary, with a uniform fulness in the front of the neck. Shortly after admission he died asphyxiated, before Mr. Bickersteth's arrival, or even before the house surgeon could be summoned. “*Autopsy*.—Soon after death a puncture was made into the floor of the mouth, when a small quantity of air and some sero-sanious fluid escaped. Thirty-six hours after death the neck was carefully examined. Nothing abnormal was discovered superficial to the deep cervical fascia, but beneath this there were evident traces of diffuse cellulitis. All the muscular interstices, and the connecting tissue surrounding the trachea were infiltrated with sero-purulent fluid, extending upwards to the root of the tongue, and downwards into the anterior mediastinum. The sub-mucous cellular tissue was also similarly affected, producing, anteriorly, sub-lingual distension, and, posteriorly, œdema glottidis. The sub-lingual and sub-maxillary glands were surrounded by inflammatory exudation, but their glandular tissue appeared perfectly healthy and natural.”

The second case was that of a lady, aged fifty, whom Mr. Bickersteth was requested to visit in consultation, and to bring instruments, as the patient was threatened with suffocation (exactly as occurred in my second case, above recorded). The symptoms were alarming, and involved originally the parts in the sub-maxillary space and base of the tongue, extending to the neck. A free incision was made, dividing all the structures and opening the fascia. The patient soon recovered.

Mr. Bickersteth's third case was that of a young man who was attacked with diffuse cellulitis beneath the lower jaw. He died suddenly of œdema glottidis before assistance could be procured.

The fourth was a very urgent case, occurring in a gentleman aged forty-eight, who was saved by a free and deep incision under the chin in the mesial line.

The fifth case was that of a young man, attacked with sub-

maxillary cellulitis when recovering from traumatic erysipelas of the thigh. He was suddenly seized with a paroxysm of choking, and died before the house-surgeon could be summoned. On a *post-mortem* examination the epiglottis was found enormously enlarged from œdematous infiltration, and there was also general infiltration among the muscles at the base of the tongue.

The sixth case occurred in a girl aged nineteen. There was a diffuse, hard swelling under the jaw. Her life was saved by a free and deep incision in the middle line between the chin and hyoid bone.

Mr. Bickersteth emphatically adds:—"We have learned by painful experience that deep-seated cellulitis cannot continue in this region without risk of œdema glottidis, and that *delay in making the proper incision for its relief may be fatal.*"

I have myself endeavoured to lay much stress on this practical fact in the observations which I have submitted to the profession on the cases recorded in this communication, and it is extremely satisfactory to me to find that the experience of so skilful and able a surgeon as Mr. Bickersteth should thus fully corroborate the important points which I am desirous to inculcate.

ART. X.—*Illustrations of the Surgery of the Femoral Artery.*

By JOHN HAMILTON, Surgeon to the Richmond Hospital, and to Swift's Hospital for Lunatics.

CASE I.—*Popliteal Aneurism cured by Compression.*

ROBERT BUTLER, aged twenty-six, a ship carpenter, a strong, tall, active looking young man, admitted May 27th, 1863, with popliteal aneurism of the right side. It is about the size of a small orange, rather at the lower part of the popliteal space, the walls of the aneurism tolerably thick. The pulsation is very strong and diastolic, so strong as to lift the stethoscope and listening head at each pulsation. No *bruit de soufflet*. Pulse 68—regular, but rather weak. Heart's action and sounds normal.

There are evidences of syphilitic poisoning; a bony node on the right tibia, at the lower third, slightly tender on pressure. A short time before it was painful, particularly at night, and tender. There are also two small, oval, yellow tertiary ulcers on the same leg. He feels only numbness in the leg, but no pain; nor is his usual activity impeded. For some time past he has suffered from numb

sensations in the limb, but he discovered the pulsating tumour only a week since, his attention having been called to it by putting one leg across the other in bed. He would feel the pulsation when he lay in his hammock, with his clothes on and his legs over the edge of it. Four years ago had syphilis; the scar of a bad bubo is apparent in the right groin. Otherwise, he is healthy, and has apparently suffered under no other syphilitic symptom, except the node and the ulcers.

The pulsation in the aneurism was readily controlled by moderate compression, on the femoral artery, both as it leaves Poupart's ligament, and at the lower end of Scarpa's space.

The man was at first very fidgetty, the upper compress could not be placed over the cicatrix of the bad bubo, which is just over the site of the femoral artery, as it crosses the ramus of the pubes, as the part became very red and tender, a portion of the instrument also got out of order, so that at the end of four days little way had been made.

Read's truss-like compressor and Carte's double compressor, in perfect order, were settled on the limb, and by alternate pressure, which was well borne and never relaxed, in twenty-four hours the tumour was quite solid.

The pressure was discontinued; to be re-applied if pulsation should return in the aneurism; but it never returned, and he left the hospital well, after a few weeks, having remained in after the cure of the aneurism, that the syphilitic symptoms might be removed by appropriate treatment.

Two years after, March, 1865, he came to the hospital with paralysis of the right side of the face, from an affection of the portio dura. The popliteal aneurism had not left a trace, and the circulation through the two lower thirds of the femoral artery was perfect.

CASE II.—*Popliteal Aneurism cured by Pressure.* Under the care of John Hamilton, Surgeon to the Richmond Hospital. By W. Thomson, A.B., M.D., Resident Surgeon, Demonstrator of Anatomy in the Carmichael School of Anatomy.

John Nicholson, aged thirty, a sailor, presented himself at the dispensary of the Richmond Hospital, on the 18th of January, complaining of a painful swelling in the left popliteal region. On making an examination, a tumour of considerable size was found in the place indicated. There was well-marked pulsation of the distensile character peculiar to aneurism. He said that he first

felt pain in the ham about the previous August. Shortly before, while pulling a hose along the deck, he became irritated because some of his comrades did not give him help, and he then suddenly jerked the tube. He immediately felt something give way behind the knee, and suffered some pain. He took no further notice of the occurrence, however, for some time; but, as he got a rheumatic affection of the other knee, he consulted a medical practitioner, and was treated for rheumatism. Two months previously to his admission to hospital he noticed the swelling of which he now complained, and as it pulsated strongly, he began to get alarmed, and at last sought advice. On further questioning him, he stated that he had served in the Royal Navy, for ten years, chiefly in China, Japan, and the East Indies. Some years ago, he contracted syphilis, and afterwards suffered from a skin eruption, but nothing more. His countenance was sallow and wasted, and his general condition anæmic. The heart sounds were normal, but there was an unmistakable bruit along the course of the aorta. The pulse was regular, but not strong.

When the patient was in bed a closer examination of the tumour showed that it extended well up into the thigh, as well as latterly; that it was thin-walled; and a measurement round the knee gave fifteen inches as compared with thirteen on the other side. Pressure on the artery stopped all pulsation in the swelling. On the following day, the patient was seen by Professor R. W. Smith, under whom he was admitted, and who determined upon the trial of pressure. He was subsequently seen by Mr. Adams and Mr. Hamilton, who concurred in the proposed treatment, although the latter expressed an unfavourable prognosis, grounded on the size of the aneurism, the thin walls, the fluid state of the blood in it, and absence of fibrous deposit. Mr. Smith, having taken ill, requested Mr. Hamilton to undertake the care of the case.

The treatment began on Wednesday morning, at 11.30. The leg was put up in a flannel roller, and a small Carte's tourniquet was applied to the femoral, so as considerably to diminish the flow of blood. He was placed upon full diet, and ordered to take an ounce of the following mixture every fourth hour:—

	Tr. Ferri perchlor.,	ʒviij.
	Tr. Digitalis,	ʒiv.
M	Aquæ ad.	ʒviij.

At 1.15 the patient declared he could not bear the pain any longer. He was very weak, and it was deemed advisable to remove the pressure altogether. O'Reilly's instrument was applied at 2.30 p.m., and was kept on, alternating the points of pressure, until 8 p.m., when Carte's was again applied at the patient's request. At 12 o'clock p.m. he declared the pain to be intolerable, and the apparatus was removed. A roll of bandage was then put over the vessel, and weights were applied. He had great relief from the change. An opiate was administered, and the patient was soon asleep.

Thursday, 23rd January.—He says the weights came off during the night, but since 4 o'clock he has held them on the vessel. The circulation is almost entirely controlled. At 1 o'clock Carte's instrument was applied, and kept on until 3.15, when the weights were again resorted to, being suspended by strong elastic from a point above. He had another opiate.

Friday, 24th January.—The patient slept well during the night. The tumour is to-day decidedly better. The pulsation is less, and deeper than it has been. There is evidently a tendency to consolidation. The weights were kept on all day. When the parts became sore the patient kept up digital compression himself. Carte's tourniquet was applied at night, instead of the weights. The point of pressure was changed by Mr Hamilton to mid-way between Poupart's ligament and the lower part of Scarpa's space.

Saturday, 25th January.—The instrument was borne well all night, the pressure giving less pain than when applied lower down. It was taken off during the day, and digital compression used for some hours, after which it was again applied. It was put on for the night at 11 o'clock, when the patient had 25 min. tr. opii. He complained very much of pain to-night. In a couple of hours afterwards the opiate was repeated, as he could not sleep. The condition of the tumour still favourable.

Sunday, 26th.—The instrument was kept on all night, but the patient did not get much sleep. The weights and digital compression were employed during the day.

Monday, 27th.—Carte's tourniquet was on all night. Throughout the day the patient was irritable, and had great pain in the tumour and in his leg. Digital compression was kept up for some hours, with an occasional return to the instrument, which was finally applied at 9 o'clock. The patient then got about 30 grains of chloral.

Tuesday, 28th.—On visiting the patient this morning he said he had slept very little during the night. The pain was more than he could bear, and he was obliged to remove the instrument altogether. He had applied it again about an hour before he was visited, but he said he was quite sure there was no blood passing through the tumour when he took off the apparatus. Mr. Hamilton saw him at 10 o'clock, when the pressure was removed, as there was no pulsation whatever in the tumour. Two articular arteries which had been observed enlarging for the past two days were throbbing strongly. The pain in the leg and tumour had considerably diminished. The tourniquet was not again applied. The patient was visited every hour, but there was no return of pulsation.

Wednesday, 27th.—The tumour still remains pulseless. Ordered mutton chops and two glasses of Marsala wine for dinner.

When this man left the hospital a fortnight afterwards the tumour was hard and contracted, and the motion of the limb very free.

The favourable termination in this case was largely due to the attention and skill bestowed on it by Mr. Thomson.

CASE III.—*Popliteal Aneurism, in which Compression failed, and in which the Artery was tied, with success.*

A large, stout, healthy-looking man, thirty-five years of age, was admitted January 22, 1868, under Mr. Adams, who transferred the case to me. He had a large aneurism of the popliteal artery, as big as an orange, pulsating very strongly. Six months previously he was making a strong exertion, during which he felt a snap at the back of the left knee, followed by uneasiness and by pulsation. Compression was tried carefully with the usual instruments, but failed. Whenever the instrument shifted, and allowed the blood to flow through the artery, or whenever it was taken off, and all pressure removed, the aneurism filled with fluid blood, and there was no indication of fibrine having been deposited. After the most watchful care for over a fortnight not only was no impression made on the aneurism, but it was evidently becoming larger. I felt, therefore, the time was come to tie the femoral artery, lest the aneurism should become diffused, and the man should have to suffer amputation to save his life. In consultation my colleagues agreed with this opinion, and I put a ligature on the vessel. In consequence of the long-continued pressure the operation was rendered more difficult and tedious than it generally is. There appeared more

congestion of the parts about the vessels than usual, and there was general and free bleeding from the wound; a small artery which was divided required a ligature. The artery and vein seemed so matted together that they required cautious separation to enable me to pass the aneurism needle between them. The aneurism at once collapsed and became pulseless after the ligature. It soon seemed to fill with blood again, though it never pulsated. The ligature came away at the right time, and the wound healed, but the aneurism continued for a long time nearly its primitive size, soft and fluctuating, and it was only two months after the operation that I thought him fit to leave hospital, the tumour having become hard and contracted. In this case the aneurism seemed to have been filled by collateral circulation, even after the main trunk was stopped.

When compression fails we naturally look for the cause. In the previous case I have expressed my belief that the failure arose from the possibility of the aneurism being still supplied with blood by some free anastomosing branch between the point of pressure and the aneurism, to which may be added a want of good coagulating power in the blood itself. Even after the artery was tied the aneurism continued for some time large and with fluid contents. In the next case, which I shall only briefly relate, the want of success was not due either to any difficult local circumstance, improper instruments, imperfect application, or the attention of the attendants, but entirely to the indocility of the patient.

A gentleman about thirty-eight years, whose character would have been most commendable but for the absence of temperance, was admitted into the hospital, as it was considered the only place to afford a chance of curing a popliteal aneurism by compression.

The tumour was situated in the right popliteal space, which it filled, about the size of a goose egg, pulsating strongly, the walls thin, and no solidity about it. For some years he had drank very hard. He had the nervous manner of one who drinks. Pulse 96; tongue yellow, with red tips and edges. The liver was enlarged. The thigh fat, with large varicose veins. The fatness of the thigh prevented compression being properly applied over the lower part of Scarpa's space, but above it, nearer Poupart's ligament, it could be well applied, and commanded the aneurism completely. In adapting the compressor the varicose veins were much in the way. It will appear, therefore, that this was not a favourable case, and the fat thigh and varicose veins, the enlarged liver, and general bad

habit of body, rendered it even more unsuitable for tying or amputation. The compression was tried for three months, from time to time only, for it had to be discontinued once for partial inflammation of the varicose veins, and at another time from an attack of acute pleurisy.

Twice the aneurism was reduced to the size of a walnut, and became solid, nor did pulsation return till some minutes after the removal of compression. A few hours more and a cure would have been certain, but these few hours were not granted. He continued to get stimulants through some of the attendants, and on one occasion was seen looking out of the window, with the compressor still on the limb. When he left the hospital he was much better in health, and the liver nearly its normal size, but he would not consent to the artery being tied.

The only effect of the compression was to delay the progress of the case, which before had been most rapid. It is now nearly two years since its use, and his medical adviser in the country informed me not long ago that he is still alive and going about.

CASE IV.—Aneurismal Varix following a Punctured Wound of the Femoral Artery and Vein—Repeated Hæmorrhage, and Ligature of the Artery. By Mr. Hamilton. Case taken by Mr. Vesey.

James Farquhar, aged twenty-four, a shoemaker, admitted into No. 1 ward, Richmond Hospital, May 25th, 1870, at 3.30 p.m., in Mr. Hamilton's absence from town. The man stated that a short time previous to admission he was cutting sand paper on a board which was resting on his knee, the knife slipped, and went with great force into his right thigh. Violent bleeding took place from the wound. He stopped it by pressure and linen wrapped round the thigh. He was then brought to the hospital.

There was a small longitudinal wound in the right thigh, at the lower angle of Scarpa's space, and about half an inch in length. It seemed deep, and the edge of the sartorius muscle was seen divided at the bottom of the wound. There was swelling along the outline of this muscle, evidently from extravasated blood. A compress and bandage were applied, and he was removed to bed.

An hour afterwards bleeding set in, and the patient lost about six ounces of blood. The wound was plugged from the bottom, and a compress and bandage applied by Mr. Henry, the resident pupil.

26th.—Spent a good night. Pulse 90. Mr. Fleming visited him, and removed the plug from the wound. He ordered an aperient,

with tartarized antimony. A pad of lint over the wound and a many-tailed bandage applied. A bladder of ice over the pad.

5 o'clock p.m.—The nurse called Mr. Vesey, as the patient was bleeding. He was lying in a pool of blood. One of the patients was directed to make pressure on the femoral artery near the groin, which controlled the hæmorrhage. The bandage and pad were then removed, and when the pressure was taken off the artery the blood was seen welling up from the bottom of the wound. Mr. Vesey tried to catch the artery with a forceps, but failed; the edges of the wound were then brought together, but this did not succeed in stopping the bleeding; the wound was, therefore, plugged from the bottom, and a compress and bandage applied.

27th.—No return of bleeding. Mr. Fleming removed the bandage, but left the compress and plug behind.*

30th.—Nothing particular since the last report; he had got some aperient medicine, but he was anxious and rather feverish; the temperature higher in the wounded limb. There is ecchymosis extending to the groin above, and below to the popliteal space.

In the evening dark pus was found to have saturated the bandage. The pad and the plug were loose, and the pus flowing freely from the wound; more was expressed by sponge and warm water.

5th June.—Nothing has occurred worth notice since the last report. But a symptom which has been observed for the last week is now very remarkable; in the diffuse swelling round the wound a very loud bruit is heard, very rough and distinct, and quite similar to that heard in aneurismal varix. A thrilling sensation is communicated to the hand.

It is now nine days since there was any hæmorrhage, but in the evening, at 6.30 o'clock, violent bleeding began, to the extent of ten or twelve ounces, saturating the bed. Pressure on the femoral artery stopped the bleeding. Petit's tourniquet was applied. The patient was very pale and in an alarming state. Pulse scarcely to be felt, livid lips, and diarrhœa. Stimulants were administered. At 10.30 a consultation was held by Mr. Adams and Mr. W. Colles (Mr. Hamilton being absent from town). It was considered the patient was in no state for operation. Mr. Carte's compressor was applied.

6th June.—Reaction has set in. Pulse 100, tolerably strong. He bore the compression well. The compressor was removed, and pads over the wound and in the course of the artery were applied and secured by broad strips of adhesive plaster and bandage.

The tumour before this was more obvious, and the pulsation remarkable; the bruit and thrill as before. He was troubled with tympany and uneasiness of the bowels. An enema relieved this. He has been taking muriated tincture of iron and digitalis for some days past.

9th.—In the evening about the usual hour, 6.15, bleeding set in, but not violently. Mr. Hamilton and Mr. Smith applied a fresh compress and bandage, and as the night was setting in when they saw the patient any operation was deferred till the next morning.

He spent a restless night, little sleep, and a strong impression that the bleeding would return, the artery "thumping strongly," and a leaping sensation from the knee up.

Hæmorrhage recurred at 8.30 o'clock, the blood coming very freely from under the compress. The hæmorrhage was restrained by digital pressure till a horseshoe tourniquet was applied.

At 9.30 Mr. Hamilton, assisted by his colleagues, Messrs. Adams, Smith, and Stokes, proceeded to tie the artery. The man was put fully under the influence of chloroform. Mr. Hamilton made an incision over the femoral artery four inches in length, including the original wound in the centre; firm pressure being made on the femoral artery by a pad in the handle of a door-key. In spite of this there was from time to time a little welling up of blood from the wound in the artery, to restrain which the pressure in the groin required to be very strong. A good deal of clotted blood had to be removed before the artery was visible; it was then seen at the bottom of the wound, quite white, with a clean cut wound in the axis of the artery, about half an inch long. The wound in the soft parts was kept open by retractors, while a strong silk ligature was passed under the artery above and below the wound, and tightly tied. All oozing of blood through the puncture entirely ceased. Some difficulty was experienced in carrying the aneurism needle between the vein and artery, amalgamated by the strong pressure that had been so long previously applied. The femoral vein, which had formed the aneurismal varix, was not apparent.

The wound was dressed with sticking plaster and lint, and a flannel bandage rolled round the limb.

On the 16th day both ligatures came away quite easily. There was a certain amount of contraction of the knee-joint remaining after the wound was healed, in consequence of the long-continued flexed position. This was remedied by keeping the limb in a double inclined splint, straightened by a screw, and he left the hospital in better health than before the accident.

.ART. XI.—*Notes of a Case Illustrative of the Advantages of Antiseptic Surgery.* By D. M. ROSS, Esq., Medical Officer Monaghan Union Infirmary and Fever Hospital; Physician Monaghan County Fever Hospital.

MARGARET M'M——, aged forty-five years, was admitted to the Monaghan Union Infirmary in July, 1872, suffering from chronic disease of the right elbow. There was a sinus leading to the joint, through which if a probe was passed, it would press against the skin on the other side, and when in the joint diseased bone could be felt. Pus was oozing through this opening. Linseed meal poultices were applied round the joint, and the patient placed on full diet with tonics. After a few days an abscess formed a little above the opening of the sinus. The abscess was opened, and in a few days it closed up. A probe could not be passed from this abscess into the joint.

The patient's general health having somewhat improved, on the 8th Nov., 1872, having obtained the assistance of Dr. Woods, who kindly administered chloroform, I amputated her arm about the centre by circular operation, and according to Professor Lister's antiseptic principles, by having a solution of carbolic acid, 1 in 40; sprayed into the wound. There were only two ligatures required, both of which were of strong unbleached thread, and the flaps were brought together with silver wire. The stump was now covered with carbolized gauze and macintosh, and a bandage applied. Very little blood was lost during the operation. A little brandy having been given, the patient was removed to bed, and shortly afterwards got 30 minims of liquor morphiae. 5 p.m.—Pulse 100; slept for a couple of hours. There has been a little oozing. 9 p.m.—Pulse 100; still inclined to sleep; no pain. Oozing has been very great since 5 o'clock. The dressing was not removed, but "antiseptic marine lint" was well placed round the stump, and at the upper end of the dressing.

9th November.—Pulse 96; slept well during the night; the oozing has completely ceased. The stump was dressed as follows: A solution of carbolic acid, 1 in 40, was sprayed over the stump while the old dressing was being removed and fresh dressing applied, which consisted of eight folds of carbolized gauze, with a piece of carbolized macintosh between the seventh and eighth layers.

10th November.—Pulse 92; slept well; tongue clean; dressed same as on yesterday.

12th November.—Pulse 96; slight redness along the edge of the upper flap; dressed as before.

14th November.—Pulse 92; the ligature of the smaller of the two vessels came away this day; the redness on the edge of the upper flap has disappeared. The suppuration is altogether limited to the edge of the wound, and is very scanty but healthy; dressed as before.

16th November.—Removed all the sutures this day; the patient was up on the 14th; dressed as usual.

20th November.—Pulse 92; wound has nearly all cicatrized; the ligature of the brachial has not come away yet, and it is only at that part that the wound is not healed.

29th November.—Every day the stump has been dressed as usual; the ligature of the brachial came away only on yesterday. The wound is now completely closed.

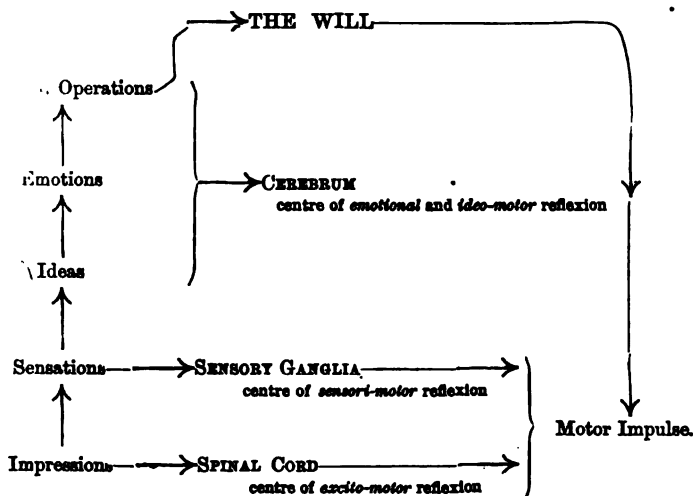
The low rate at which this patient's pulse kept during the cicatrization of the wound (92 is her normal pulse), the absence of furring of the tongue and of constitutional disturbance, and the fact that she was able to be up and dressed on the sixth day, sufficiently attest the advantages of the antiseptic method.

ART. XII.—*Observations on a Case of Aphonia.* By THOMAS DAWSON FINUCANE, L.F.P., S.G., &c., &c.

I FEEL encouraged to bring the following case of recovery of the voice in a lady under the notice of the medical profession, as there are some particulars connected with it which may prove interesting. Being called in to see this lady, I had great difficulty in discovering the nature of her ailment, owing to her complete inability to utter the slightest sound, and I was subsequently informed by her sister that she also had been similarly affected for a period of at least twelve months, and although she had been under the treatment of the most eminent physicians she failed to recover her voice until she had complete change of air, to which she ascribed its restoration. In the treatment adopted towards my patient I found it necessary to order a stimulating anodyne liniment to be rubbed along the lumbar region, and curious to relate, on repeating my visit the following morning I was agreeably surprised to find the

stored. The patient thus related what
 out was being applied:—I felt (she said)
 my spine until it arrived opposite the
 for a second. I then felt it again ascend
 the back of the head, when suddenly I got
 .., and exclaimed, "How strange; I have

seemed to me so extraordinary that I sought
 object, and was not disappointed, as the follow-
 Carpenter's valuable work will plainly show.
 actions of the Nervous System he says, "The
 put forth in regard to the independent and
 of the several primary divisions of the *cerebro-*
 may, perhaps, be rendered more intelligible by the
 which is intended to represent the ordinary course
 when the whole is in a state of complete functional
 character of the reflex actions, to which each part
 when it is the highest centre that the impression can



We have, I think, an accurate drawing of the above table in the
 simple language used by the patient. Beginning with the impres-
 sion produced by the liniment, we have a something ascend; in fact,
 a sensation along the spinal cord, and although it hal

second, it terminates in the idea of speech and intellectual operation in conversation. In the above case the will could not accomplish the production of any sound until it was stimulated by the reception of a sensation conveyed to it by the cord, and I cannot do better than again quote from the same able authority, who says, "No definite tone can be produced by a voluntary effort unless that tone be present to the consciousness during an interval, however momentary, either as immediately produced by an act of sensation, recalled by an act of conception, or anticipated by an effort of the imagination. When thus present the will can enable the muscles to assume the condition requisite to produce it, but under no other circumstances does this happen, unless the particular mode of discipline by which the congenitally deaf may be trained to speak."

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The West Riding Lunatic Asylum Medical Reports. Edited by
J. CRICHTON BROWNE, M.D., F.R.S.E. Vol. II.

WE have much pleasure in noticing the second volume of the West Riding Lunatic Asylum Medical Reports, edited by Dr. Crichton Browne, and in bearing our testimony to the value of its contents. These show how indefatigably the gentlemen connected with this institution are labouring to extend and give precision to our knowledge of insanity, and to urge forward the study of many collateral branches of inquiry.

Space will not permit us to enter at any length into the many interesting questions opened up in these papers. We can only direct attention to a few of the salient points in the volume.

Dr. Wilkie Burman, in a paper on the subcutaneous injection of conia, gives an account of a number of experiments, not only upon the lower animals, but upon the human subject, by means of which he has arrived at the following conclusions:—Pure conia is useless as a hypodermic injection, giving rise only to an abscess at the seat of injection; but, neutralized with hydrochloric or acetic acid, it is very efficient. When given pure and by the mouth conia acts as a powerful irritant, producing, in addition to the ordinary symptoms of hemlock poisoning, excoriation of the tongue, foaming at the mouth, and vomiting. Neutralized and hypodermically injected, these latter effects are not produced. The *post-mortem* appearances after poisoning by conia coincide in the main with those observed after death by asphyxia. Dr. Burman has injected himself and twenty-two other healthy adults on fifty separate occasions, the doses ranging from *one-twelfth of a grain* to *two and one-fifth minims* of conia, neutralized and in solution. The symptoms experienced were very uniform, always making their appearance in from five to fifteen minutes. After m. ij. of conia Dr. B. has perceived confusion of vision, numbness and tingling of the arms, heaviness of the eye-lids, unsteadiness of gait, and thickness of the

voice, accompanied by a general feeling of tranquillity. Three hours after the injection he felt perfectly well, and none the worse, eating a hearty dinner, and finding the digestive functions unimpaired. A curious result of these injections was that while in from a quarter to three-quarters of an hour after injection the strength of the patients, as measured by the dynamometer, was reduced to a minimum, the patient pulling from six to eight stones *less* than when in ordinary condition, in two hours after injection it sometimes increased to a maximum, the patient pulling from one to three stones *more* than when in ordinary condition.

Dr. Burman has found the subcutaneous injection of conia of great service in cases of acute mania in which the motor excitement is great, and where the patient refuses food. He gives a number of illustrative cases. He considers conia especially valuable as a sedative, from its not causing congestion of the brain, or headache, or affecting the digestive system. He recommends forced exercise as a valuable method of treatment in conia poisoning. He hopes to find "an antidote for acute mania" in the combination of morphia and conia hypodermically injected. Messrs. T. & H. Smith's conia is the best which he has yet procured.

Dr. Herbert C. Major contributes an interesting paper "on the minute structure of the cortical substance of the brain in a case of chronic brain wasting," in which he states that the healthy brain exhibits "a delicacy and distinctness of structure" which is wanting in the diseased brain. He appears to think that the difference is easily recognizable by means of the microscope.

Dr. H. Sutherland shows the connexion between menstrual irregularities and insanity. He believes the amenorrhœa of general paralysis to be due to diminution in calibre of the vessels of the uterus simultaneously with a similar change in the vessels of the brain.

Dr. Samuel Mitchell gives the results of a number of experiments with ether and nitrous oxide combined, upon the lower animals and the human subject. He finds in this combination a safe, expeditious, and pleasant anæsthetic.

The greater part of his paper is taken up with a theory as to the mode of action of poisons. He thinks that our views on the subject of the mode of action of anæsthetics are somewhat confused. He attacks Dr. Anstie's position, that "most, if not all, narcotics, when given in smaller doses than those required to produce the lesser degrees of narcosis, act as stimulants;" and attempts to show further, that the time-honoured opinion that certain drugs have

"elective affinities for certain tissues in preference to others" is without sufficient foundation.

We are not sure that we quite comprehend Dr. Mitchell's theory, which we should like to see more clearly and completely stated; but the general drift of his arguments would appear to be as follows:—The question as to the mode of action of anæsthetics is a portion of the wider question as to the mode of action of poisons in general. This he accordingly proceeds to discuss. He shows what confusion of opinion the "elective affinity" theory has given rise to, the action of strychnia being localized in the sensory nerves, the motor ganglia of the heart, the spinal marrow, the tissues and fluids of the body, &c., by various acute observers. He is disposed to accept Dr. G. Harley's opinion, that most poisons act by destroying the power of the tissues and fluids to absorb oxygen and exhale carbonic acid—a view which gains, he thinks, some support from Kussmaul's and Tenner's comparison of the action of rapidly fatal doses of most poisons, to that of copious and sudden hæmorrhage, and their suggestion that this resemblance was due to the sudden interruption of the nutrition of the brain in each case. We presume that Dr. Mitchell tacitly assumes that the functions of the brain are dependent upon the absorption of oxygen and exhalation of carbonic acid, which is interfered with, either by the withdrawal of blood or by the destruction of power in the fluids and tissues by poison. He can scarcely mean that he accepts the term "nutrition" as synonymous with "absorption of oxygen and exhalation of carbonic acid." His general conclusion is that *poisoning* in all forms may be considered as equivalent to *arrest of oxidization of tissue*, but we cannot see that he has made any serious attempt to show how the various effects of different poisons can be accounted for on this theory.

A considerable portion of the paper is taken up with an attack upon the current ideas with regard to stimulants. Dr. Mitchell disputes Dr. Anstie's opinion, that the effects of true stimulants, such as alcohol, opium, and tobacco, are similar to those of food, and asks whether a substance which in small quantities acts as food can in a little larger doses become deadly poison. He thinks it much simpler to suppose that all stimulants or sedatives (whichever we choose to call them) act on the tissues as uniformly as an acid or an alkali, and "disable the powers of life," by "destroying the power of the tissues and fluids to absorb oxygen and exhale carbonic acid." What is called "stimulation" is, in fact, only a

form of poisoning. Food alone is really stimulant; alcohol, &c., are nerve paralyzers.

The action of anæsthetics he considers to be analogous to that of bodily exhaustion or nervous fatigue, and the excitement which sometimes follows their administration is due (if we understand him rightly) to irritation *ab externo* of the nerve-centres, paralysed and enfeebled by the anæsthetic.

Dr. Mitchell has, doubtless, got hold of some germs of truth, but we must protest against the manner in which he has rushed into print with a tissue of crude ideas and ill-considered arguments. We should feel much obliged to him if he would supply the missing links in his train of reasoning, which contains gaps which we find it difficult to fill up, and we strongly recommend him to be a little less vague in his use of such terms as *nutrition*, *oxidization*, *irritation*, *stimulation*, &c.

The editor contributes a very interesting paper on "Cranial Injuries and Mental Diseases," in which he states that the records of the asylum tend to show that injury of the forehead is extremely perilous, injury of the side of the head less so, injury of the back of the head quite as hazardous as that of the forehead. Injury of the frontal region leads to development of epilepsy and epileptic mania. He traces this to small hæmorrhages in the medulla, produced by *contre-coup*, and states that oxen felled by the poll-axe are invariably convulsed, and that he has found such hæmorrhages in them after death. He thinks that there are facts which tend to support the theory that the central lobe of the cerebellum is the seat of sexual desire. Extraordinary salacity in aged persons is sometimes associated with defective co-ordination, and in these cases disease of the central lobe extending to the hemispheres has been found in the cerebellum. He speaks at considerable length on the subject of change of character produced by cranial injury, and suggests that the career of some hardened and incorrigible criminals may be due to some such cause. "There is no phase of derangement which may not have a blow on the head for its starting point." Idiocy, recurrent mania, dementia, dementia with epilepsy, senile dementia, and *mania e potu*, are the most frequent traumatic forms.

Dr. G. H. Pedler points out the value of active purgation in puerperal mania.

Dr. Herbert C. Major describes a new instrument for determining the depth of the grey matter of the cerebral convolutions,

and gives tables of measurements in various localities, in acute and chronic mania and in senile dementia.

Dr. Nichol's paper on the mental symptoms of ordinary disease—a most important subject—is not quite satisfactory. He confines himself too much to cases in which there is some distinct cerebral lesion. He speaks, indeed, of the effects on the mind of the condition of the nutritive system, and connects stupidity with *ozæna*, irritability with deafness, mildness with blindness, an anxious manner with cardiac disease, hopefulness with phthisis, depression with dyspepsia, hysteria with genital disorders, &c.; but all this is rather vague.

There are several other valuable papers in the volume, which want of space prevents our noticing as they deserve. Dr. Maziere Courtenay speaks highly of the benefit resulting from the use of opium in melancholia; and we may say that some of our Dublin physicians can bear a similar testimony to the value of this treatment.

Dr. A. F. Browne tells of some strange cases of impairment of language in cerebral disease, and is inclined to consider the quasi-religious jargon of Irvingism, &c., as a form of aphasia, or, at least, related to it.

Dr. Thompson gives sphygmographic tracings taken from epileptics, with short notes of the cases. In one of these the patient had a seizure while the sphygmograph was actually at work, and the tracing shows the effect of the *status epilepticus* most distinctly. This is accompanied by "a lax condition of the vessels."

Altogether, we may congratulate the gentlemen connected with the West Riding Lunatic Asylum on the amount of good solid work which they have been doing there of late.

On the Scientific Value of the Legal Test of Insanity. By J. RUSSELL REYNOLDS, M.D., F.R.S., &c., &c.

DR. REYNOLDS'S object in this paper is, in his own words, "to examine the scientific value of the legal tests of insanity; to show that the two professions, the legal and the medical, which frequently come into collision on this matter, approach it from points of view so widely separated, that their united action is, in many cases, quite impossible;" and that this is so because the lawyer and the physician

hold altogether different ideas as to the meaning of the word insanity.

The object of the two professions in any legal inquiry is the same. "It is not to determine what insanity is; nor what are the signs of its presence; but whether certain tests applied to the case in question will place the individual in the category of the sane or insane." Each profession has its own idea as to the nature of insanity, but unfortunately these ideas differ. Now, the medical profession regards insanity as the result of physical disease, with "a definite origin, causation, development, and history;" whereas the legal distinctions are "artificial lines, untrue to nature." Dr. Reynolds, therefore, suggests a conference between the two professions, "with the purpose of devising means for bringing the legal criteria of insanity into harmony with the facts of science."

The paper is divided into three parts. In the first Dr. Reynolds examines "the legal doctrine with regard to insanity generally;" in the second, "the legal test of insanity when urged as a plea of innocence in criminal prosecution;" in the third, "the legal doctrine in respect of mental capacity in those who are held to be 'partially insane.'"

I. The legal doctrine with regard to insanity generally is that, in the words of Shelford:—"A sound mind is one wholly free from delusion; an unsound mind, on the contrary, is marked by delusion;" a delusion being "a belief in facts which no rational person would have believed." But regarded from a scientific point of view this is not a correct definition. "It is incorrect," says Dr. Reynolds, "because many undoubtedly insane persons have no delusion;" and even where delusion is present it is frequently "scarcely separable from the errors of judgment committed by those who are indisputably sane." As an instance of insanity without delusion he adduces melancholia, in which the patient may be driven to suicide by his sensations of overwhelming horror, without ever having given evidence of anything like delusion. It is only the skilled physician who can recognize the insidious approach of such a malady as this. The *per saltum* lunacy of the lawyers is never found to occur in practice, and to arrive at a correct diagnosis in any case it is necessary to consider carefully a number of slight indications which, for the ordinary observer, have little significance. Cases in which insanity seems to burst out suddenly, may be compared to cases of sudden death from fatty heart. The diseased condition gradually supervenes, until some

slight extra strain upon the system results in the terrible catastrophe. We can no more determine accurately the period of origin of the cerebral than we can that of the cardiac disease.

The legal definition of delusion is, moreover, extremely vague. Shelford defines it "as a belief of facts which no rational person would have believed." But who can decide about or how much a "rational person" may believe? Some are naturally credulous, some sceptical. Some believe only in the dicta of their own intellects; others pin their faith to some authority. And who is to say when the feeling of personal sinfulness, which we are taught to regard as the commencement of a religious life, becomes excessive and morbid, and tends to that condition of abject misery so distressing in some forms of lunacy?

II. The legal test of insanity, when urged as a plea of innocence in criminal cases, is the proof that "the accused was labouring under such a defect of reason, from disease of the mind, as not to know the nature and quality of the act he was doing, or, if he did know it, that he did not know he was doing what was wrong."

But this test is untrustworthy. There are, of course, lunatics in whom the moral sense is blunt or extinguished, but in thousands of cases the very essence of the disease is a morbid exaggeration of this sense with regard to their own conduct. The moral sense may, in fact, be very keen, and yet perverted with regard to particular acts or courses of conduct. Between the two extremes of lunatics who have lost the moral sense, and those in whom it is morbidly keen, there are all degrees of change both in quantity and quality; so that "it is simply monstrous that the 'knowledge of the difference between right and wrong' should be made the test of insanity."

Morbid *impulses* must, in fact, be taken into consideration in determining the question of responsibility. An unstable condition of the nervous centres which allows of impulsive action often leads to suicide or homicide.

III. In the third section Dr. Reynolds inquires "What are the tests of partial insanity? and what are the legal relationships of the so-called 'partially insane?'" He objects to the legal doctrine that partial insanity may be held sufficient ground for depriving a man of his rights as to the disposal of property, and yet insufficient to excuse him from the responsibility of a criminal act. The legal tests are here also inadequate; the phrase "of unsound mind" being used in an arbitrary and objectionable manner as equivalent to the term "lunatic," whereas, in many instances, the unsoundness

merely amounts to an incapacity for attending to complicated affairs, management of property, &c. At present the law is such that a person certified to be of "unsound mind," in order to relieve him of responsibilities too heavy for him, is shut out from the performance of the most ordinary duties of life, for which he is quite fit. Just as if a man were certified to be "of unsound heart," to save him from the risk of running races or going up mountains, and henceforward, on the strength of this certificate, kept constantly in bed lest he might walk quietly on the level. Every case should be judged on its own merits.

In conclusion, Dr. Reynolds suggests the following points for consideration at the conference which he proposes between the legal and medical professions:—

1. The arrival at a better definition of insanity generally.
2. A revision of the tests of insanity; (a) that based upon the existence of delusion; (b) that turning upon the knowledge of right and wrong, and of the consequences of actions.
3. An examination, in all its bearings, of the doctrine of partial insanity, and its responsibilities.
4. A revision of the distinction between responsibility for criminal acts and capacity for civil acts.
5. An inquiry into the mode of dealing with those whose mental condition is impaired, but who are not, in the popular sense of the word, "insane."
6. An examination of the possibility of dealing with those bordering upon insanity as at present recognized.
7. A determination of the mode to be adopted in dealing with cases, both civil and criminal, when insanity is alleged as a plea of innocence, or as a bar to disposing power.
8. The possibility and desirability of doing away with the present mode of investigation in a court of law—viz., by the calling of skilled witnesses on different sides.
9. The possibility and desirability of a court or of a commission to report in all cases of impending legislative inquiry, in regard of those who are alleged to be insane.

We trust that this paper, which deals so exhaustively with a subject of such great importance, may lead to some practical result; and we believe that the conference which the author proposes would be a step towards a modification of the law in accordance with the dictates of philanthropy and scientific enlightenment.

Ozone and Antozone: their History and Nature. When, Where, Why, How is Ozone observed in the Atmosphere? By CORNELIUS B. FOX, M.D., Edin.; Member of the Roy. Coll. Phys., Lond.; Fell. Brit. Meteor. Soc., &c. London: J. & A. Churchill. 1873. Pp. 348.

It is strange that notwithstanding all that has been said and done during the past quarter of a century or so respecting the mysterious entity to which the illustrious Schönbein gave the name ozone, no book, properly so-called, on the subject, had seen the light until the year 1872 was drawing to a close. On a fellow subject and professional brother the task has devolved of analysing the detached literature of the subject, and of thoroughly investigating it for himself in a searching, a scientific, and a practical manner. This task, no light or trifling one, has been ably accomplished by Dr. Cornelius Fox, of Scarborough, who has evidently regarded it, not as such, but rather as a pleasure.

The work opens with a brief historical sketch, commencing with the Homeric description of a thunderbolt, ἐν δὲ θεοῦ πλῆτο, and travelling down to the present time. As regards this history, it is merely necessary to recall that Schönbein's researches subsequent to the year 1840 marked an epoch in the history of the substance he first termed "ozone" (ὀζω—"I smell"), and which he believed to be simply oxygen in a *negatively* polar state, while "antozone" similarly was oxygen in a *positively* polar state.

In the first division of his book, Dr. Fox answers the question, "What is ozone?" He believes that Schönbein's hypothesis just mentioned must be given up, and he accepts without reserve Dr. Odling's theory, as suggested by an observation of Andrews and Tait, and as confirmed experimentally by M. Soret. It may not be without interest to our readers to give a short account of this theory. Messrs. Andrews and Tait, in experimenting on oxygen, found that a constant and considerable diminution of volume accompanied the conversion of oxygen into ozone by the electric discharge—in fact, 100 volumes of oxygen when subjected to the silent discharge contracted to about 92 volumes; only $\frac{1}{12}$ th of it, meanwhile, being converted into ozone. It was thus shown that ozone was denser than oxygen. Again, if the ozone set free in this way was absorbed by the introduction of an oxidizable substance into the ozonized oxygen, the residual oxygen was still found to have the same volume as it had before the removal of the ozone. Lastly, if the ozonized oxygen were heated (ozone being

destroyed by heat), the original 100 volumes of oxygen were obtained. Of these remarkable facts Dr. Odling suggested an explanation based on the modern theory of *atoms* and *molecules*. Oxygen being expressed by the formula O_3 , ozone, according to Dr. Odling, would be O_3 or O_2O , its formation meaning simply the condensation of oxygen to the amount of one-third, and its oxidizing power depending on the facility with which it loses its third atom of oxygen. M. Soret's experimental verification of this theory is thus described by Dr. Fox:—

“He (M. Soret) has discovered that, whereas most substances remove only the third atom of oxygen from ozone, oil of turpentine is capable of absorbing *the whole molecule*. If the 92 cubic inches of ozonized oxygen are treated with oil of turpentine instead of with mercury, a white cloud is produced, and the residual oxygen is found to occupy a volume of only 76 cubic inches. The only possible explanation here is, that the 92 cubic inches consisted of 16 of ozone (O_3) and 76 of unaltered oxygen (O_2), and that the former was seized upon entire, and removed in the solid form by the oil of turpentine” (p. 14).

Ozone, then, is an allotropic form of oxygen, its formula being $O_2O = 48$. It may be obtained by eight different processes, all of which are fully described and explained, while a valuable table giving the relative properties of ozone and oxygen is appended.

The answer to the question, “What is antozone?” is by no means of so satisfactory a nature, but all that is known on the subject is concisely stated in one chapter, the generally received opinion being that antozone is a mixture of peroxide of hydrogen and air.

The second part of the work is devoted to the consideration of the question, “When is ozone observed?” In seeking for an answer, Dr. Fox appears to have consulted every available treatise on the subject, and to have taken great pains in reconciling conflicting statements of different authors. In many instances an apparent diversity of opinion as to what are the ozone-bearing winds has been removed by an inquiry into the relative positions of an observer's station and the sea—that great manufactory of ozone. As a general rule, northerly winds are deficient, while southerly winds are rich in ozone.

“Speaking generally,” observes Dr. Fox, “it may be said that ozone is contained in the air in larger quantity during the winter and spring months than during the summer and autumnal ones. Why? Winter and spring are especially characterized by rain, snow, hail, a maximum of electricity, low temperatures (hence a minimum of decomposing and

noxious principles), and high winds. The great activity of vegetable life during the latter season must not be forgotten.

"Summer and autumn are, on the contrary, distinguished by high temperatures, a maximum of air-pollution, owing to the decomposition of a comparatively large amount of animal and vegetable matters, by a minimum of electricity in the lower atmospheric strata, and by the infrequency of gales" (p. 88).

The author then passes to the third division of his subject, "Where is ozone observed?" The answer is given thus (p. 93):—

"Ozone is found in greater abundance in pure country air than in impure town air, on mountains than in valleys, at the seaside than inland, in well drained and ventilated towns than in those where these important sanitary matters are neglected. It is nature's great deodorizing and purifying principle."

At the same time it is suggested that an *excess* of ozone may possibly exercise an influence for evil on living plants and animals. In support of this, the destruction of leaves by a northerly gale at Scarborough, in the beginning of the summer of 1870, is instanced, hesitatingly no doubt. For our own part we must regard this point as "non-proven." If ozone in excess thus shrivels up vegetation, we should look for a desert along our southern and south-western seaboard. At the same time ozone *plus* cold may have this hurtful influence.

"Why is ozone observed?" is the fourth question with which Dr. Fox deals, and this portion of his work deserves most careful perusal, for in it is contained a *résumé* of our knowledge of the relations between ozone and disease. The sources of ozone are summarized at page 124:—

"The oxidation of metals, the decomposition of rocks, the germination of seeds, the growth of plants, the falling of dew, rain, hail, and snow, the collision between air-currents of different degrees of humidity proceeding from opposite quarters with one another or with the earth, the evaporation which is continually proceeding from saline fluids, such as oceans, seas, and lakes, the dashing and splashing, the smashing and crashing (!) of the restless waves on the rocky coast, are all concerned in the simultaneous development of electricity and ozone."

After giving in detail the views of nearly all the writers on the subject as to the influence of ozone in controlling or promoting disease, Dr. Fox concludes that—

"1. A deficiency of ozone in the air in all probability predisposes to disease, particularly of the epidemic form, by virtue of the depressing and debilitating effects of such air, in consequence of its feeble powers of oxidating animal *debris*; and

"2. A permanent diminution in the normal amount of active oxygen probably favours the development of chronic diseases characterized by mal-nutrition, imperfect oxidation, and degeneration of tissues" (p. 151).

The uses of ozone are succinctly enumerated a few pages further on, the restoring of freshness to meat and the value of ozonized oils in consumption being those most closely allied to medicine.

In the concluding portion of the work the query, "*How is ozone observed?*" is answered, and we really must congratulate Dr. Fox on the manner in which he has handled this division of his subject. To speak of it as an exhaustive treatise in the fullest sense is no more than this part of his work deserves. All the accepted tests for ozone are described at length, their relative deficiencies and merits are considered, and the methods of their application with the effect of external influences upon them are explained and exemplified. The different aspirators in use (the principle common to all being to draw over the test papers a known and fixed amount of air) are also described and represented by carefully executed drawings.

In concluding this necessarily incomplete review of Dr. Fox's work, we have only to express the pleasure with which we have read it through, and to add that a clear and faultless letterpress, good paper, and beautifully drawn lithographs are enough to tempt a purchaser apart from the intrinsic value of the contents of the volume.

WORKS ON MEDICAL THERMOMETRY.

1. *Medical Thermometry and Human Temperature.* By C. A. WUNDERLICH and EDWARD SEQUIN, M.D. New York: Wood & Co.
2. *The Clinical Thermometer: its Lessons and Teachings tentatively expressed in Numbers. Report of Committee on New Remedies to the Muskingum County Medical Society, 1871.* By T. C. M'ILROY, M.D. New York: Baldwin & Co. Pamphlet.

DR. SEQUIN's book consists of two parts—the first, a valuable epitome of Wunderlich's work on Medical Thermometry; the

second, various remarks by Dr. Sequin himself, entitled "Suggestions on Thermometry and Human Temperature."

As we have so recently laid before our readers a *résumé* of the chief features of Wunderlich's great work in our notice of the New Sydenham Society's English translation by Dr. Woodman, we need not again remind them of the chief facts of medical thermometry, which are now so well established.

We regret extremely that Dr. Sequin should have thought it necessary to burden his valuable and well-arranged extracts from Wunderlich's work, which forms a complete manual of medical thermometry, with his own suggestions, given as they are in a not over modest style. There is one suggestion of Dr. Sequin's which we must protest against, and that is—the proposal to establish another thermometric scale, on the ground that our present thermometers are not medical. Dr. Sequin proposes to establish a scale for medical thermometry, with the point of normal temperature as the zero of the scale, using the centigrade degree. The advantage of this arrangement Dr. Sequin seems to us to fail to prove, and he appears to forget what he had previously pointed out, that the exact temperature of the healthy body has not yet been accurately fixed; indeed, we do not see how it can be, as many slight differences must arise, according to age, sex, and situation, which would involve a different zero for each condition. We do not see why a *medical* thermometer is a necessity. A good thermometer, with a fine column, so as to give a long range for each degree, made as short as is consistent with accuracy, is all that is required for clinical use, and the less curiosity and speciality we introduce into thermometers for medical use, the more likely we are to secure accuracy. Dr. Sequin also goes into the question of surface thermometers, but throws no new light upon this interesting question.

Dr. M'Ilroy's pamphlet is chiefly of the nature of a philosophical discussion of the question of medical thermometry. We would point specially to Dr. M'Ilroy's remark, that "the efforts of empiricism to connect specifically different pathological states with certain degrees of temperature have up to this time clearly ended in failure." And again, "Philosophy with wider vision confirms the statements of science," and further adds, that, "in organic life, temperature is wholly due to retrograde chemical metamorphosis, and, with almost absolute certainty, to the retrograde metamorphosis of solid tissue."

The above quotations point to the method with which Dr. M'Iroy approaches the subject of medical thermometry; and although Dr. M'Iroy's remarks may not be of a very practical character, yet they are of a nature which unfortunately is too uncommon in medical literature, especially in that of America and England.

The results of Dr. M'Iroy's reasoning and observations are thus summed up at the conclusion of his report:—

"1st. That the clinical thermometer, in connexion with previous history and present rational symptoms in any given case whatever, . . . is in the hands of the actual working practitioner . . . an instrument of as much precision and certainty as any employed by civil engineers or mechanics in our time.

"2nd. That the clinical thermometer, considered singly, holds the first and highest place among all the means at the command of the actual working practitioner for making positive diagnosis, based on the actual condition of molecular work, or motion in the living body, in pathological states, with ranges of temperature above or below the physiological standard.

"3rd. In like manner the clinical thermometer, considered singly, holds the highest place among all the means at the command of the actual working practitioner for making correct prognosis in pathological states, with ranges of temperature above or below the physiological standard.

"4th. That the clinical thermometer, considered singly again among the means at the command of the actual working practitioner, holds the highest place for unerring therapeutical guidance in all pathological states, with ranges of temperature above or below the physiological standard.

"5th. That the numbers in the table or on the thermometer scale, opposite to those marking degrees of temperature, represent tentatively the value of variations of temperature in pathological states ranging above or below the physiological standard, reference being had to the points at which molecular forms of structure are losing or lost, and consequently death."^a

We are glad to see, from the two works before us, that medical thermometry is making good way among our American cousins,

^a Dr. M'Iroy has arranged a table as above, in which he tentatively fixes numbers corresponding with the degrees on the thermometer (Fahrenheit) scale, "marking the regular geometrical increase or decrease of *motion* towards loss of molecular forms of structure, and death." These numbers pass upwards to 512, corresponding with 108°, marked: "losing molecular structure," "probable death," and to 2048, corresponding with 110° marked: "molecular forms of structure lost," and "death." Similar numbers and denominations occur at 93° and 92° respectively.

and we have no doubt that their natural ability for turning new discoveries to practical account will soon be demonstrated in this as it has been in so many other arts and sciences.

WORKS ON MATERIA MEDICA.

A Year Book of Therapeutics, Pharmacy, and Allied Sciences.

Edited by HORATIO C. WOOD, Jun., M.D. New York: W. Wood & Co. 1872. Pp. 360.

IN the present volume, which from its nature scarcely admits of criticism, the editor classifies his material under five heads, viz:—Therapeutics, *Materia Medica*, Toxicology, Prescriptions and Formulas, and General Receipts. Certainly the propriety of introducing the last two sections is at least questionable, and throughout the rest of the work not only is the arrangement careless, but the references to authorities are loosely given, and in many instances no dates are furnished. The utility, indeed, of year books is daily becoming less appreciated, now that abstracts and reports on the various branches of medical science are so extensively adopted in all the leading periodicals; still, for those who desire to possess in a single volume an epitome of recent inquiries, Dr. Wood's year book will step in to supply their needs.

Aide-Mémoire de Pharmacie; Vade-Mecum du Pharmacien à l'Officine et au Laboratoire. Par EUSEBE FERRAND; avec 184 figures. Paris: J. B. Baillière et Fils. 1873. Pp. 687.

M. FERRAND undertakes in this volume to furnish students and pharmaceutical chemists with a compendium which may serve as a handy-book of reference for those who have scanty time for study, and few books at their command. The work, which does not profess to be anything more than a compilation, is arranged alphabetically, and each article is discussed concisely as to its pharmaceutical history, physiological action, therapeutic uses, and chemical and toxicological relations. Numerous formulæ and prescriptions are given, and the text is illustrated by 184 woodcuts of apparatus, plants, and objects of interest in animal chemistry.

The Pharmacopœia of the United States of America. Fifth decennial revision. Philadelphia: J. B. Lippincott & Co. 1873. Pp. 383.

EVERY ten years a National Convention is appointed for the revision of the United States' Pharmacopœia, and the present edition is the outcome of the labours of a committee delegated by the Convention, which met in Washington in 1870, to carry the work into execution.

The first thing that strikes one in glancing over this volume is its similarity in size and form to the British Pharmacopœia, and this likeness extends deeper than the cover, for the arrangement, like our own, is alphabetical, and is, indeed, more consistently so; the typography is excellent, and the characters and tests of the drugs are printed in small clear type. The materia medica lists, primary and secondary, are kept distinct from the preparations, and the former category, which contains the definitely recognized drugs, includes few articles of importance not to be found in our national code.

No chemical symbols are given, an omission which we see no reason to justify; volumetric solutions are not used, but, in the case of acids, the weight of alkaline bicarbonate which will neutralize a certain weight of the acid is stated. We notice with pleasure that Dr. Attfield's suggestion for a more uniform chemical nomenclature has been adopted. Thus *all* the salts of the alkaline metals are designated as of the particular metal, and not of its oxide, *e.g.*, *calcii carbonas*, *potassii citras*, and so by this simple modification the nomenclature of all the metallic compounds is unified.

The old standard of troy weights is adhered to; twenty-seven articles have been added to the materia medica lists, and five have been discarded, while to the preparations eighty-two have been added, and seven dismissed. Following the lead of the British Pharmacopœia, we find under the head of new classes of preparations *chartæ*, *glycerita*, *suppositoria*, and *succi*. A large number of new fluid extracts is introduced, and in the preparation of most of them glycerine is employed in connexion with alcohol, by which the latter is much economized, and objections to former processes are removed. The committee have, we think, acted wisely in not lending their sanction to any scheme of doses, and several comparative tables of measures and weights and an accentuated index, for the benefit of indifferent scholars, conclude a work which is in every way a creditable reflex of the existing condition of medicine and pharmacy among our American brethren.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Wednesday, 9th April, 1873.

DR. STOKES in the Chair.

THE LATE DR. EAMES.

DR. LOMBE ATTHILL said before they proceeded to the business of the night, he begged, as one of the oldest and most intimate friends of the late Dr. Eames, to ask the Society to express its deep sense of the loss it had sustained by the death of its Honorary Secretary. By a strange coincidence that was the anniversary of his marriage—a sad anniversary to his poor widow, who had but a few days ago given birth to a posthumous child. Dr. Eames was beloved by all from his frank, cordial character, from his kindness, and from the anxiety he ever evinced to make those who came into contact with him happy. He was but a short time, comparatively speaking, a member of that Society, but during that short period he became well known to all of them. At a very early period he became Honorary Secretary, and during the time he acted in that capacity he discharged his duties with great efficiency, and the Society had sustained a great loss by his death. He begged to move the following resolution:—"That this Society desires to express its sense of the great loss it has sustained by the premature death of Dr. Henry Eames, who for four years filled the office of Honorary Secretary of the Society, and to convey to his family its sincere sympathy with them in their bereavement."

DR. WHARTON, in seconding the resolution, said he had had the happiness of a long acquaintance with the late Dr. Eames and his family, and could testify that in all the relations of life in which he knew him, there was no one more deserving of esteem than their lamented Secretary.

The CHAIRMAN, in putting the resolution, which was adopted, said he agreed with everything which had been said by the mover and seconder.

He, himself, was long acquainted with Dr. Eames, and always admired his gentlemanlike and social qualities.

On Two Cases of Poisoning by the Fumes of Charcoal. By J. HAWTREY BENSON, M.B., Dub., Physician to the City of Dublin Hospital.

ONE morning lately I was hastily summoned to a house in one of the suburbs of Dublin with a message that two of the inmates could not be roused out of their sleep. On arriving I found the sufferers were young men, University students, of the age of eighteen each. They slept in the same room, but in different beds. The room was very small, and the fireplace had been stopped up; consequently ventilation was most defective. For five nights previously they had a movable charcoal stove brought into their room while going to bed, for the weather was cold, but this was carried out when they were in bed. However, on the sixth night the servant did not remove the stove, and the two lads went to sleep in happy ignorance of the terrible consequences they were about to encounter.

CASE I.—When I entered the room I found that one of the lads had already been roused, but was still heavy and confused, and was quite unable to appreciate what had happened. His pulse was then 104, small and feeble; temperature $99^{\circ}.8$; respiration 20. He complained of slight headache. Presently he had some slight vomiting, which seemed to relieve him, for it was only after that he regained full intelligence. He remained in bed all that day, and by next day he was nearly quite well, though somewhat weak.

CASE II.—But the condition of the second lad was far more serious. I found him deeply comatose. The face was livid and bloated; the respiration laboured and stertorous; the eyes congested; the pupils at one time contracted, at another dilated; slight tendency to froth at the mouth; teeth firmly clenched; some rigidity of the limbs; pulse 140, but quite regular and of fair strength; temperature 103° ; respiration 36; some increased area of dulness on percussion over the right heart, with very visible jugular pulsation; and when any part of the surface of the body was pressed with the finger the white persistent impress indicated the sluggishness of the peripheral circulation; no dulness on percussion at the back or base of the lungs, but very numerous dry râles all over.

Finding the patient in this condition I immediately threw up both windows, and opened the door, so as to let a thorough draught sweep through the room, and drew out the bed into the middle of the floor. I then applied mustard to various parts of the body, and as I happened to have Gaiffe's induction apparatus with me at the time, I passed the current between the phrenic nerve and diaphragm during the inspiratory move-

ments, with the hope of its acting not only as a special, but as a general stimulant. The only apparent effect of this was to make inspiration somewhat deeper, and afterwards to induce hiccough. Finding no perceptible improvement from these measures I next sent for my colleague, Dr. Purser, and at his suggestion we administered a cold affusion of water at about the temperature of 60°.

At our next visit, at one o'clock, we found the pulse 132, temperature 102°.6, respiration 36. There was then very profuse perspiration, the drops running off his skin. But as there was no decided improvement in his symptoms, we agreed to ask Dr. Hudson to see him. Accordingly, at 3.30 we met, and, as Dr. Hudson thought there was still evidence of considerable distension of the right cavities of the heart, he advised to have the patient blood-cupped under each clavicle. We did so immediately, and with difficulty succeeded in removing three or four ounces of blood.

At 9.30 that same evening Dr. Purser and I saw the patient again. He was still deeply comatose; pulse 120, temperature 102°, respiration 24. A catheter was then passed, and about a pint of fluid removed.

With a view of obtaining some direct evidence as to the condition of the cerebral circulation Dr. Purser made an ophthalmoscopic examination of the fundus of the eye, and found a condition of extreme venous congestion.

The night was passed tranquilly, but the patient continued deeply comatose. About 5 o'clock next morning he had slight vomiting. Very soon after that he began to show signs of intelligence, and between 6 and 7 o'clock a.m. he might be said to have regained full consciousness; that is about 30 or 31 hours after he began to inhale the poison.

At 10.30 that same morning Dr. Purser and I saw him again. He was then perfectly intelligent, but suffered very great muscular prostration. He was quite unable to turn in bed, or to help himself in any way, and it was with the greatest difficulty that he succeeded some hours afterwards in expelling his urine. The tongue was quite dry and brown in the centre; pulse 104; respiration normal.

In the evening we found slight tendency to congestion at the back of left lung; the dry, bronchial râles, which were so numerous the previous day all over both lungs, had nearly disappeared; muscular prostration still extreme; pulse 120; temperature 102°.4; respiration 20.

3rd day.—Passed a tolerably tranquil night; slight delirium; had considerable flying pains in the feet and the calves of both legs; no appetite; tongue dry and brown; sordes on lips and teeth; considerable thirst; pulse 88; temperature 99°.4; respiration 20; limbs still so powerless that when lying on his back, with the clothes stripped off, quite unable to draw up his knees in the bed. In the evening pulse temperature 99°.6.

4th day.—All symptoms diminished; pulse 88; temperature 98°.7.

5th day.—Pulse 80; temperature 99°.2.

6th day.—Pulse 78; temperature 99°.

7th day.—Pulse 76; temperature 97°.

8th day.—Pulse 72; temperature 98°.4.

It was not until the 9th day that his strength admitted of his getting up.

Such cases as these, though by no means so common in this country as on the continent, are yet of sufficient frequency to demand the full attention of the practitioner, while to the physiologist they are replete with interest.

Among the first considerations which strike one in connexion with these cases is the very unequal effects of the poisonous gases on the two lads—one so slightly affected, the other so gravely. There was only a few months difference in their ages, they were both lying at the same level above the floor, and the effect of the charcoal stove having been placed a few feet nearer one than the other would, I think, be counteracted by the rapid diffusion of the gases. The difference may, perhaps, be ascribed to idiosyncrasy.

As to the more prominent symptoms, such as the high fever, the extreme muscular prostration, the profuse sweating, the muscular and neuralgic pains, &c., but especially the remarkable persistency of all these, they become most interesting when viewed by the light of modern investigation as to the mode in which the products of the slow combustion of charcoal produce their deadly effects.

It has long been known that carbonic oxide, though formed in less quantity than carbonic acid during the combustion of charcoal, is far more deleterious in its effects. But it is since the writings of Claude Bernard on the subject that the precise manner in which carbonic oxide operates on the living body has been ascertained, and the marked difference in the duration of the effects of this gas and of carbonic acid explained.

While Bernard's view as to the mode of action of an atmosphere charged with carbonic acid has been confirmed, viz., that its presence (when not in sufficient quantity to cause spasm of the glottis) prevents the elimination of the carbonic acid already in the venous blood, and excludes oxygen, thereby causing asphyxia without any fresh introduction whatever of carbonic acid into the blood—while this has been confirmed a further light has been thrown upon the behaviour of carbonic oxide when brought into contact with blood. The oxide when inhaled becomes chemically combined with the hæmoglobulin of the blood, forming a definite chemical compound termed carbonic-oxide-hæmoglobulin. This new substance has different optical properties from either the oxy or the reduced hæmoglobulin, for it possesses a definite

spectrum. It is also found *not* to possess the power of entering into loose chemical combination with oxygen, and imparting the same to the tissues. It possesses, therefore, no respiratory properties whatever. Consequently, a quantity of effective hæmoglobulin proportionate to the quantity of carbonic oxide inhaled and combined is lost to the system, and is not only lost, but becomes a poison circulating in the blood.

The effects of these chemical changes in the blood are then seen in symptoms such as those presented in these cases.

In the meantime the unimpaired hæmoglobulin is yielding up oxygen to the carbonic oxide, converting it into carbonic acid, which, ceasing to exist in chemical union with any part of the blood, is conveyed in solution in the watery part of the blood to the lungs, and is there eliminated as in ordinary respiration. But as the oxidation of the carbonic oxide is slow, so the recovery of the patient is equally slow.

This slowness of recovery contrasts strongly with the effects of carbonic acid, which, if they are not immediately fatal, are comparatively transitory, owing to the fact that the gas, not forming any chemical compound in the blood, is rapidly eliminated when the patient is removed from the circumstances which caused its accumulation in the blood.

DR. PURSER, having been alluded to by Dr. Benson, said he had merely to deal with the fundus of the eye. The appearances were such as to lead him to the belief that the venous system was gorged, and the arterial system was empty. The optic entrance was red, and the appearances were not different from the normal condition.

DR. HENRY KENNEDY, having complimented Dr. Benson on the admirable manner in which his paper had been prepared, said he recollected when an apprentice, seeing a case brought into hospital of a man who had fallen asleep close to a lime-kiln, and who sustained a severe burn sufficient to produce a slough, and finally great disease of the bone, rendering trephining necessary. When brought in the man was entirely comatose, and it was very remarkable that there was an immense amount of sordes around his lips. He had all the symptoms detailed by Dr. Benson, the surface being exceedingly livid, and the condition of coma intense. The man came out of this state in a day, but he was deranged for a considerable time afterwards, rambling in his head. Another case which had come under his observation, was that of an old lady, who slept in a very small closet, and who was affected through the fumes of a fire arising in an adjoining room from a turf fire in the grate. He was not chemist enough to say whether it was the same gas which was produced; but the result was the same as in the cases detailed by Dr. Benson. He used constant friction in that case. The lady came out of the coma in three days, and she also raved for some time afterwards.

DR. QUINLAN said he had seen another method of treatment adopted towards the dog experimented upon in the grotto at Naples. The animal had been held rather longer in the grotto than usual, and when taken out he lay for some minutes on the bank as if dead, and showed no signs of coming to. The man then took the dog by the hind legs, and gave him two or three rapid dips in a neighbouring pool, which soon brought the animal round, and he ran away apparently as well as ever.

The CHAIRMAN fully agreed with what had fallen from Dr. Kennedy, as to the admirable manner in which these two cases had been presented to the Society. They were model cases of an accident fortunately rare in this country. He himself had seen little or nothing of it. He remembered long ago the case of an unfortunate man who slept on a lime-kiln near the Meath hospital. He slept with his head towards the fire, and did not awake, and when brought into the hospital the whole of the posterior part of the skull and scalp were deeply burned. He did not think the man ever showed any signs of recovery, and on *post-mortem* examination the bones were found to be literally calcined. One great point of interest in these cases was the production of a fever clearly produced by a direct poisoning of the blood. On the subject of cold affusion just alluded to, he might mention a circumstance communicated to him by a pupil of his, the late Dr. Bradshaw. That gentleman accompanied the 90th regiment from Cawnpore to Lucknow, and on that occasion they had the greatest proportion of cases of sunstroke on record. The regiment marched in the night to escape the heat of the sun. They outmarched their camp-train, and when they arrived at their camping ground they were long ahead of their baggage waggons. The exhausted men flung themselves on the burning ground and fell asleep, and when the trumpet-call sounded a great many of them made no response. They were examined and found in a state of profound coma. The coma in these cases was generally fatal, unless in cases in which the treatment directed by the Medical Board of India was neglected. All the cases that were treated *secundum artem* died. In the cases in which recovery occurred there was nothing done whatever in relation to the directions from head quarters. What was done was simply cold affusion to the head, continued for a long time; and patients who were in such a state of insensibility and collapse, that sensible persons declared there was no use in doing anything for them, recovered when treated in that manner. The late Commander of the Forces in Ireland, Lord Strathnairn, in one of his forced marches, was particularly liable to sun-stroke, and was able to know when the attack was coming on. He had a corps of men numbering thirty attending him, each of whom carried two goat's skins of water; and whenever Lord Strathnairn felt the action of the sun on his head, he would pull up and throw himself

on the ground, and these men would throw all this enormous quantity of water on him, and he would get up and proceed on his march. This occurred three times in a day's march. With reference to the terrible cases of sunstroke mentioned by Dr. Bradshaw, although the lives of the men were saved they were hardly fit for soldiers afterwards; and in the course of a year, or a year and a half, they had to be invalided from some nervous lesion, want of memory or some symptom of paralysis, when they were all sent home, and discharged from the army.

DR. BENSON said that he saw one of the young men alluded to in his communication a few weeks afterwards suffering from a copious crop of boils, and this in connexion with the blood poisoning, was an interesting fact.

Pulmonary Hæmorrhage a Cause of Consumption. By J. MAGEE FINNY, M.B., Univ. Dubl.; Fellow King and Queen's College of Physicians.

THE subject of pulmonary hæmorrhage as a cause of consumption, which I have the honour of bringing under the notice of the Society this evening, I shall best introduce by detailing briefly the following cases:—

CASE I.—Miss F., aged eighteen, while in the act of dressing previous to going out for a walk on the afternoon of July 9th, 1869, spat up, with a slight cough, a quantity of bright blood.

This patient, a young lady of prepossessing but delicate appearance, and very tall for her age, had never before suffered from any serious illness, except that, as a child, she had had a chronic abscess over the left shoulder, nor, at the time of the hæmoptysis, had she been in other than her usual health, the catamenia being always regular, the last monthly illness three weeks ago. The evening preceding she had walked a couple of miles to her uncle's house in the suburbs of Dublin, and had spent the evening playing with her uncle's children, with, it was remarked, more than usual vivacity.

Her family history plainly indicated a tendency to pulmonary phthisis, as her father had died of that disease twelve years before, as well as one of her elder sisters, three years ago, while another sister was threatened with the same fourteen months ago, and was at this time troubled with a fistula in ano.

When I visited Miss F., about an hour after the occurrence of the hæmoptysis, I found her greatly agitated and alarmed about herself, her face very flushed, pulse 120, and with a short irritating constant cough. She complained of no acute pain in the chest, but of a slight aching sensation under the angle of the left scapula. No abnormal sound was elicited by percussion in any part of the thorax over the lungs, while a slight moist râle was audible over the back part of left side, but without

either tubular breathing or crepitus. This râle I attributed to the blood, which probably was lodged in the small bronchi. Ice was given to suck, and 8 grain doses of gallic acid with 5 min. of tinct. aconite and tinct. opium every second hour. This was followed by the best results, for in eight hours, at my evening visit, there was no bleeding, and the pulse had fallen to 100—a condition which continued throughout the next day.

On the 11th July the pulse rose to 120, and the cough was very constant and troublesome. I again made a careful examination, without finding any comparative dulness in any part of the lungs; the respiratory murmur was, however, more distinct over the right than the left side. Posteriorly in the left on full inspiration a muco-crepitant râle was audible, while under the left clavicle the respiration was *entre-coupe*, and a bruit was noticed in the sub-clavian artery. The gallic acid mixture was omitted, and a sedative mixture containing acid hydrocyan., digitalis, and vin. ipecac. substituted.

The night following, hæmorrhage returned in greater quantity, when nearly a pint of bright frothy blood was expectorated (about perhaps 8 oz. of pure blood). It was preceded by no exertion on the patient's part, and occurred very quickly, lasting only about half an hour. Twenty minims of oil of turpentine were given every hour for three hours, and then the gallic acid mixture was resumed.

In the morning (12th) I first took the temperature, and found it marked 100°.5; pulse 125, and respirations 28. There had been no more bleeding, but sickness of stomach, occurring after some beef-tea, brought up a quantity of black matter, not unlike coffee grounds, which was most probably blood that had been swallowed. In the afternoon, unfortunately, hæmoptysis recurred, and fully 6 oz. of blood were lost. She was then quite blanched, with white gums, and the least elevation of the head was followed by symptoms of syncope.

I had now the advantage of Dr. Head's advice, and at his suggestion we employed 20 min. doses of liq. ext. ergotæ three times a day with marked benefit. With the exception of a couple of ounces the next morning (13th), and blackish sputa for a few days (up to the 19th), there was, from this time to the end, no return of active bleeding from the lungs.

An examination of the front of the chest failed to discover anything abnormal, and on the 15th the report is the same; while, posteriorly, beyond a little crackle at the end of inspiration at the inferior angle of the scapula, there was nothing abnormal observable. On the 18th, that is, nine days after the first attack of hæmoptysis, percussion posteriorly, for a space extending from 1 inch above the inferior angle of the scapula to 2 inches below it and coming forward to the posterior fold of axilla, elicited a comparatively, but decidedly, dull sound, while tubular

breathing, bronchophony, and moist large rales were heard with the respiratory act; more, however, with inspiration than expiration. On the right side, almost to the same extent, there was dulness with coarse sub-crepitant rale on inspiration.

The fever, as shown by the thermometer, had risen now to 103°, with variations. Pulse 120-124, and respiration 28-32 in the minute. The fever was evidently remittent, as in the evenings the face would greatly flush, and great heat would be complained of. The patient complained of no pain, except an occasional slight stitch under the left breast, and she could lie equally well on either side. The bowels were costive, and the skin was always very dry. Decided emaciation, with much weakness, was now only too apparent. Menstruation, however, appeared on the 17th, and lasted, but to a small quantity, till the 19th. On this day the expectoration became extremely pneumonic, very viscid, so as to adhere to the vessel when upturned, and a little rusty, and this character it presented for seven days, till the 24th.

On the 22nd the thermometer marked a slight improvement, but the physical examination showed that the lung seemed to be breaking down, and the disease to be extending round to below the mamma on the left side. As yet, no dulness under the clavicles was discovered, but interrupted respiration, which had been absent for some days back, is again heard under the left.

Not to delay longer than is necessary on this case, I may briefly state that the lung-changes went on from bad to worse, as might be expected, but the right lung seemed after a while to be even more extensively engaged than the left, the destructive process being principally located in the posterior parts, and the last place where the signs of breaking down of the lung were evidenced, was the left apex.

The patient died on 4th September, fifty-seven days from the hæmorrhage of the 9th July. The treatment, in a word, consisted of sedatives, tonics, and supporting diet, including fatty matters, such as cream, butter, cod-liver oil, and pancreatic emulsion, and, when all these failed, the fat of bacon. This latter, I may remark, I have found to agree on other occasions in similar cases.

CASE II.—Miss M., aged twenty-two, naturally pale in complexion, but who, previous to this attack, had generally enjoyed good health, suffered from moderate pulmonary hæmorrhage on the night of 27th January, as she was undressing to go to bed. The bleeding was not profuse, and had ceased when I arrived, shortly after midnight, but still, whenever she coughed a small clot was brought up. Pulse 110, respiration 40 in the minute. She complained of nothing but occasional shooting pains under left shoulder, and was, as might be expected, in a state of great alarm and nervous excitement. On inquiry I found that one of this

lady's brothers had died of hæmoptysis and dropsy three months before—the disease producing such symptoms was not known—and that another brother was just recovering from a severe attack of pleuropneumonia. There was, however, no history of tubercle in either father or mother, nor in the members of any near branch of the family. Physical examination elicited slight dulness under the left clavicle, with absence of the vesicular murmur, and the presence of slight tubular breathing. As the catamenia were expected, I entertained hopes that the hæmoptysis was but part of the vascular excitement which accompanies that period, and ordered perfect rest and quiet, eight-grain doses of gallic acid every third hour, and a draught of twenty-five min. of tinct. opii at once. The bleeding did not return during the night, but did shortly before my visit in the morning: not, however, to any amount. The dulness under the clavicle was certainly less marked; due, no doubt, to the gravitation of the blood backwards. Pulse 100, respiration 40, and temperature 99°. As the bowels were constipated I gave her sulph. magnesiae in acid infusion of roses, and in addition pills of plumbi acet. c. opio, and applied a small blister to the left subclavicular region.

29th January.—No bleeding since last report, only occasionally black clots are brought up. The catamenia have come on. Her lips are dry, tongue coated, the bowels costive. The pulse, respiration, and temperature and other symptoms much the same, but there was more pain complained of under left scapula. In addition to slight clavicular dulness, dulness is found in the posterior inferior part of left lung, where there is almost total loss of the respiratory murmur. As she spoke so very low, the physical sign of vocal fremitus could not be tested. Ordered to continue saline purgative, and to be dry cupped over left side of chest.

30th.—She feels better. Respiration still quick (33-40), and speech interrupted; pulse 104; temperature 100°; a crop of herpes, three in number, has appeared on lower lip. The pain in her chest is less, having been greatly relieved by the cupping, and she can lie on either side without distress. The dulness posteriorly is more marked, and over the whole lower lobe there is absence of respiratory murmur, while it is exaggerated in the right lung. The heart's sounds were very audible in left axilla and under left clavicle. A diaphoretic mixture, consisting of liq. ammon. acet., spir. ætheris nitrosi, vini antimon., in camphor water was ordered; beef-tea, and ice to suck.

The pneumonic change in the left lung was still more plainly evidenced next day (31st January), by decided dulness in posterior inferior part; absence of all breath sounds in this part, and the presence of bronchophony. The clavicular dulness has greatly cleared up. Little expectoration, and then only a few black clots, had occurred, evidently the remains of the blood, which four days ago had been poured out into the

smaller tubes. Pain is again complained of under the scapula. Pulse 120, respiration 40, temperature 99°.

Ordered, to continue the diaphoretic mixture, and to have a jacket poultice enveloping the thorax.

In the evening she was worse, as her breathing, though 40 when lying, was greatly accelerated on sitting up, with the *alæ nasi* moving at each act of respiration, and she complained of a sense of great oppression in the chest. With Dr. Banks' advice, she had fifteen min. of tinct. *digitalis* added to each dose of the mixture, and flannel wrung out in hot water and covered by oil silk was applied to the chest.

February 1st.—Pulse 124, respiration 40, temperature 100°.25. Cough very slight, and only two or three sputa during the night, which had the same black character as the day before.

2nd.—Pulse 100, respiration 30, temperature 98°; the breathing is much easier; the pains are not complained of; no cough or expectoration. The dulness posteriorly is less marked, and the air seems to enter the left lung in a couple of spots, where crepitus could be detected. The *digitalis* was now omitted, and a quinine mixture given. The chest to be reddened with turpentine twice a day. Ordered an egg, wine, and beef-tea freely.

5th.—The pulse rose yesterday to 120, respiration 40, and temperature 100°, and to-day the pulse is 126, respiration 30, temperature 100°. After a restless night and day her stomach became sick, and she coughed up a few *bloody black sputa*, that is, nine days after the first bleeding. Under ten min. doses of tinct. *digitalis* the pulse came down next day to 106, respiration 28, temperature remaining the same. No more vomiting nor hæmoptysis. Physical examination elicited a flat sound under left clavicle, where the vesicular murmur was very faint, but neither tubular nor crepitant râles were audible. The lung behind is much clearer, and both fine and coarse muco-crepitant râles are audible over the back. Things now seemed to improve, and resolution to set in, for in three days (9th) the temperature fell a degree, 99°, respiration 26, and the air was entering the lung more freely, and with much less crepitus.

February 12th.—Pulse 92, respiration 24, temperature 98½°. Percussion was natural over the whole of the lower left chest except for about two square inches at the bottom, where it continued dull; but in left supraspinous fossa comparative dulness was observed. On 15th she had improved so much as to be able to sit up every day, her appetite having returned. Pulse 84, temperature 98°, and almost no expectoration. The lung, however, in spite of this improvement, had not regained its normal character; there was still dulness posteriorly and inferiorly, and also a flat sound in supraspinous fossa, while on deeper inspiration a clicking mucous râle was audible in the latter situation, and crepitant râles in the former.

Nocturnal sweating has set in also the last couple of nights. On the 19th, after this temporary improvement, she seemed to fall back, and softening was manifestly going on in the lung, both the apex and base, for the râles heard were both coarser and more muco-crepitant, and the dulness below had extended up for about three inches, and was spreading into left axilla. Bronchophonic voice was very evident also in the same region. The patient was evidently in hectic. Pulse 104, temperature had risen to 101°.5.

21st.—In two days pulse rose to 120, respiration 30, and temperature 101°.5. Expectoration now became more profuse and easily brought up; rusty in colour, but lacking viscosity.

26th.—Pulse 138, respiration 32, temperature 103½°. The whole left lung behind and in front was quite dull, with muco-crepitant and large crackling râles, except for a space of about 2½ inches below the spine of left scapula, where there were tubular breathing and bronchophony.

This state of things continued, but daily becoming worse, until she died, on the 13th of March—just forty-four days from the occurrence of the bleeding of the lungs on 27th of January.

CASE III.—The third case is shortly as follows:—A young gentleman (Mr. A. G.) aged twenty-one, a medical student, and a friend and pupil of my own, had always enjoyed excellent health, up to the autumn of 1867. He was of a healthy family, residing in one of the southern counties, and had been accustomed from his boyhood to all the field sports of the county, and would often walk eight or ten miles a day, simply for exercise. There was no history of consumption in his parents, who are alive, nor in any ancestor or relative, that I could discover on strict inquiry.

In August, 1867, this gentleman, of robust and vigorous frame, was out shouting grouse with a companion. He had cleared a large fence at a leap, and came down, as he told me, rather heavily on his feet. Almost instantly he coughed up a few expectorations of bright blood. He did not at once discontinue his walk; but after a little, being alarmed at the sight of the blood, he returned home, and took some saline cathartics and remained quiet for a couple of days on very low diet.

As the cough did not, under this treatment, diminish but rather increased, and some distress was felt low down in the right side, the local doctor was called in to see him, and he treated him for pneumonia, by antimonials, mercurials, diaphoretics, and blisters. Being only a little better, he came to town in the end of October for advice, and I had the advantage of Dr. Stokes' and Dr. Hudson's opinion on the case. It was only too apparent that his right lung was extensively engaged, part of the lower lobe being in a state of consolidation, and the apex in a state of sub-acute inflammation. His symptoms also plainly pointed to phthisis

in its early stages. He was advised to return to the country. For a time, under full and generous diet, with cod-liver oil, he seemed to improve, but towards the middle of December he seemed to catch cold, and the consolidation of the lower lobe became more absolute and extended. Unfortunately the same lowering treatment as at the first was employed—blisters, mercury, and low diet, being resumed. When I saw him on 8th January, the lung tissue was breaking down fast, and vomicae forming, but still chiefly in lower and posterior part of inferior lobe rather than in the apex, where the progress of disease seemed all through to be very slow. I at once succeeded in putting a stop to the treatment and tried to support him by nutritious food and wine. After a rally for about a fortnight, he failed again, and died the beginning of February, five months from the occurrence of the hæmoptysis.

Remarks.—These three cases coincide in one remarkable and important peculiarity—namely, that the lungs did not exhibit, on examination at the time of the initiatory hæmorrhage, any signs of previously existing or commencing organic disease in the lungs or elsewhere, and that it was not until some days had elapsed that the symptoms and signs of inflammatory changes in the lung tissue gradually developed themselves.

The first point of interest which claims our attention is, I conceive, the manner in which the consumptive process began in each of these cases. In all, the individuals were young and, speaking generally, in good health. They were able to pursue their ordinary avocations up to the day and hour of the bleeding, and even to undergo considerable muscular exertion and fatigue. In all, the hæmoptysis came on suddenly without any symptom, referable to the chest, having been complained of or noticed; and although in Case III. the bleeding was to a very small amount, in all it was the foundation of, the starting-point of the future consumptive process.

This process I have intentionally termed “consumptive,” in preference to “tubercular,” for two reasons: first, because the latter term may convey so very much or so very little, as the discussion at present proceeding in the Pathological Society of London on this subject well exemplifies; and, secondly, because, in the absence of *post-mortem* confirmation, to state definitely what the particular disease of the lungs was, would be but to dogmatize.

As these cases are but a few examples of many similar cases, which no doubt the experience of each of my hearers could readily supply, it is not uninteresting to make inquiries as to the probable causes of such bleeding before I allude directly to my views of the relation the hæmorrhage bore to the disease of the lungs. As explanatory of so untoward, and to the patients and their friends so alarming, a symptom, I would mention three views, having for their basis three different

conditions. The first—that there existed in these otherwise healthy and to all appearances, sound individuals, prior to the occurrence of the hæmoptysis, a diseased state of the walls of the arteries of the lung; the second—that tubercles, deposited in the lung and lurking there for an indefinite period, produced the bleeding; and the third—much more simple than either—that under some undue exertion on the part of the patient, there occurred an accidental rupture of some blood-vessel, either bronchial or pulmonary.

There is another pathological condition capable of explaining the sudden occurrence of pulmonary hæmorrhage, to which I shall merely allude, as being, I consider, outside the scope of the subject to which I would confine my consideration in this paper—namely, the pulmonary aneurism of Rokitansky, which has been so fully worked out by Rasmussen of Copenhagen, and Dr. Peacocke.

The first view—that of a previously diseased state of the pulmonary arteries—is no doubt very ingenious, and one which, if we could adopt it, would smoothen many a difficulty and elucidate many a dark recess in the etiology of this, and allied, abstruse subjects. Dr. Williams, whose work on Pulmonary Consumption contains so much valuable and highly practical matter, adopts this view. In all cases of large and early hæmoptysis he believes the vessels have been, previously to such hæmorrhage, in a state of fatty degeneration. He writes thus:—“When hæmorrhage (from the lungs) be more than a drachm in a person free from hæmorrhagic diathesis, cancer, disease of the heart, injury of the chest, disease of the uterus, it is indicative of a fragile state of the vessels closely connected with and arising out of consumptive disease of these organs;”^a and this condition on the one hand, and on the other pulmonary aneurism, he states “to be the only explanation of such profuse hæmorrhages in the first stage, with slight consolidation of the lung, when the general health is otherwise apparently good.”^b

Dr. Williams seems to base his view upon the microscopic investigations made by Dr. Radcliffe Hall, the correctness of whose observations I should be sorry to question in the smallest degree; but it occurs to me that the opportunities of microscopically examining the lungs by Dr. Hall, or others, in the very beginning, the outset of consumption must have been so few that the statement lacks all other weight than that it derives from the names of such men. For this reason, and also because the adoption of such a view would only drive us to inquire what could be the cause of the fatty degeneration of the blood-vessels in such young subjects, and thus we should be no nearer the truth, simply substituting one difficulty for another, I have great hesitation in accepting such an hypothesis, for the bleeding occurring in the very beginning of

^a Pulmonary Consumption. Williams. 1871. P. 144.

^b *Ibid*, p. 145.

the case, prior to all fever or bronchial irritation. Were it the end of the first stage instead of the beginning, of which I spoke, nothing would more readily and fully explain the occurrence of hæmorrhage, inasmuch as in the caseous degeneration which follows upon catarrhal pneumonia, or in the retrograde progress of tubercle (as some believe), the lungs are exactly in the condition in which we would expect to find the smaller vessels in a state of fragility, lying as they do in the midst of, and pressed upon by, the granular and fatty matter of the surrounding degenerating hyperplasms. That this condition, at that period of consumption, does exist seems fully borne out by the experience of Drs. Williams and Pollock, as shown by their statistical returns. In the Brompton Hospital the fact was elicited, and a remarkable fact it is—that hæmorrhage is proportionately more frequent in the first stage of consumption than after softening has set in, as 3 to 1.

To my mind there seems no occasion to seek so elaborate a theory in explanation of early hæmoptysis, and can only attribute it to the impress the doctrine of Laennec has made upon such writers, so that they cannot divest their minds of it, or accept inflammation as one, at least, of the causes of phthisis. To the exclusive advocates of such a view I would simply ask, Do you deem it necessary to seek such a theory of a previously morbid condition of the vessels to explain the occurrence of epistaxis, which so commonly is met in young people, at a like age as these cases of pulmonary bleeding?

With this theory of fragility is closely connected the second view I have mentioned, namely, that advocated by Laennec, that prior to any signs of inflammation, there exist tubercles lying in the lung, and ready to show their presence when the patient is placed in such conditions as will favour their development. On this point I shall not delay, as it is opposed to the pathological views which have been so widely adopted during the last eight or nine years. And even, I would say, if we were to adopt this view, their existence could hardly enter into our consideration as a sufficient explanation of the hæmorrhage—profuse hæmorrhage in some instances—which introduced the cases I have detailed.

The third theory of early hæmoptysis, namely, that of accidental rupture of some vessel in the lung, commends itself to me, both from its very simplicity and the parallel it has in the occasional bleeding from the vessels of the nose, or from piles during puberty, that period of vascular excitement. Bleeding from the bronchial mucous membrane, Niemeyer has noticed as being very frequent between the ages of fifteen and twenty-five in those patients whose parents were consumptive, and who themselves suffered from rapidly growing tall.

In a recent article (which has appeared in the *Journal de Médecine de l'Ouest*, 1872), M. Trastour endeavours to show that hæmoptysis occurs

quite independently of the existence of pulmonary phthisis much more commonly than has been supposed. These hæmorrhages he terms "congestive hæmoptyses," and classifies their origin under three heads:—

1. Those which are accidental, the result of violent effort or of contusions of the thorax.

2. Those which appear to be of nervous origin.

3. Those which result from some temporary hyperæmia or inflammatory condition of the lungs or bronchi.

This author admits that in very exceptional cases the extravasation of blood into the pulmonary tissue may be a cause of phthisis; and, inasmuch as his adoption of Niemeyer's doctrine of "phthisis ab hæmoptoë" is so very restricted, I consider his opinion deserving of more weight than if he had been a warm supporter of such views.

This simple theory of accidental bleeding, to my mind, fully explains its occurrence in the cases detailed. In Case III. hæmoptysis followed upon jumping a fence and using more than ordinary strain and muscular power, and might come under the first class of M. Trastour; while in Case I. it is probable it was induced by the long walk and exertion of the evening preceding, aided doubtless by the vascular excitement attendant upon the menstrual period which had arrived. This latter, I believe, may be considered as operating in Case II. also as an exciting cause.

I would now enter upon the second and chief point of interest suggested by these cases, viz., the relation, if any, the pulmonary consumption bore to the hæmoptysis.

In the present unsettled and often contradictory views of pathologists on the subject of tuberculosis, and while the committee of the Pathological Society of London, under the able guidance of Dr. Wilson Fox, is holding its investigations and discussions upon this subject, I consider it not inopportune to bring this all-important topic before the Society tonight—important alike to the scientific man and to him whose chief concern is the therapeutics of phthisis; and while I would venture to express my own views as to the relation of hæmorrhage and pulmonary consumption, views which, in my limited experience, must necessarily carry little weight with them, I do so principally with the object and desire to elicit the opinions of those who have had much experience in this disease, and who, by their position, abilities, and opportunities, are so much better qualified to form an opinion on so vital a question.

Inasmuch as I have already stated that as far as I can read the cases detailed, the occurrence of the bleeding was purely due to accident, it is unnecessary to say that I consider the pulmonary consumption which caused the death of these young persons was the direct consequence of such hæmorrhage; that the hæmorrhage and the inflammatory action set up in the lungs bore to each other the relation of cause and effect.

The steps of the morbid process, I would suggest, were in somewhat this order:—First, the bleeding—part of the blood was got rid of by expectoration, but part remained behind in the smaller bronchi, and either by gravitation or the inspiratory acts it became lodged in the alveoli of one or more lobules. Here, either by its presence as a foreign body, or by the retrogressive changes it underwent, it produced irritation and the second step or inflammation. This inflammatory change seemed from its rapid growth to be that form termed by Niemeyer “acute catarrhal pneumonia,” in which occurs a too rapid development of the alveolar epithelium with its accumulation in the alveolar cavities. This, at first confined to a lobule or a group of lobules, spread through various parts of the lobes, where it became recognizable by well-marked physical signs. Owing to the malnutrition of the parts thus consolidated and to the pressure of these rapid growths upon their vascular supplies, the third step, or “softening with ulceration,” was produced, the patients being literally consumed of the high fever which attended these morbid lung-changes.

On this point the views of the great German pathologist, Niemeyer, are as follows:—

He says, “Capillary hæmorrhage, either bronchial or pulmonary, does not infrequently lay the first foundation for pulmonary consumption in persons in whose lungs neither tubercles nor pneumonic deposits previously existed. This is brought about by the blood which remained behind in the alveoli, as well as the products of the inflammation which the blood caused, undergoing cheesy metamorphosis. In the same manner do bronchial and pulmonary hæmorrhages not infrequently accelerate the course of an already existing pulmonary consumption.”^a

It is unnecessary for me to add other words expressive of the same view, as I entirely agree in the present instance with the doctrine of *phthisis ab hæmoptoe*. The objections to Niemeyer’s doctrine have hitherto been, I believe, twofold, and are, I presume, still likely to have many supporters. The one advanced by those maintaining the doctrines of Laennec on the non-inflammatory origin of tubercle, the other by those who, holding the same views, take a narrower field of objection in asserting that consumption cannot be produced by bleeding into the alveoli, for, say they, such hæmorrhage does not and cannot produce inflammatory action.

I will first meet the latter objection by drawing attention to analogous effects, on both mucous and serous membranes, the result of inflammation, excited by blood effused, and exposed to the air. As a familiar example of its effects upon the mucous membranes I would refer to the well-known danger of leaving a plug soaked in blood in contact with the mucous

^a Text Book of Practice of Medicine, Niemeyer, p. 33, Am. ed., 1870.

membrane, be it of the nares or vagina. It is not alone the plug that the surgeon dreads, but it is the effused blood, as he knows full well that such blood, changed perhaps and in the process of decay, will occasionally set up inflammatory action, and still further, may render products which, if absorbed, will poison the whole system, and be evinced by serious constitutional disturbance. On this point Professor Gross writes thus:—

“I have seen several cases where from this cause (that is plugging the nares for epistaxis) the patient lost his life, being seized with a low form of fever, attended with delirium, which nothing could arrest. In one of the cases there were marked symptoms of pyæmia.”^a The testimony of Dr. Paget on this point is as follows:—“If (*blood be*) effused in large quantity, so as to form a voluminous clot, and especially, if so effused in a wound which is not perfectly excluded from the air, or if effused in even a subcutaneous injury in a person whose health is not good, the blood is most likely to excite inflammation, and the swelling of the wounded parts or their commencing suppuration will push it out of the wound.”^b

To the accoucheur the same is well known, and for like reasons, the danger of any blood clots being left in the uterus after parturition is universally recognized.

Why, I would ask, should the same be denied to the mucous membrane of the bronchi? Is there anything structurally or physiologically different which would warrant us in exempting it from a like danger to which other mucous membranes are exposed?

Effusion of blood on a serous membrane seems to be comparatively innocuous, but this holds good only so long as air be excluded. The observations and experiments on blood effused into the plural cavity, as given by Trousseau,^c fully bear me out on this point.

For these reasons, based on analogy, as well as for the conviction which has been forced upon me by a careful examination of cases published similar to those related, as, for instance, those by Niemeyer, Drs. Bäumler and Weber, and others,^d I hold that blood, be its source bronchial, as Niemeyer maintains, or pulmonary, as Rasmussen holds, which has been effused into the bronchi and alveoli may be, and sometimes is the cause of setting up inflammation of these parts.

I should, however, be sorry to be thought to limit the origin of phthisis to this cause, or even to accept the teaching of Niemeyer as to its being a frequent cause, or to deny that on the one hand hæmorrhage may occur without causing any further change, or on the other, should even inflammation be set up by its presence in the neighbouring tissues, that the

^a Prof. Gross, *Syst. Surg.*, Vol. ii., p. 334.

^b *Surgical Pathology*, Paget, 1st ed.

^c *Clin. Med.*, par A. Trousseau, prem. tome, p. 738,

^d *Clinical Society's Transactions*, Vol. ii., pp. 82 and 143.

products of such inflammation, which are chiefly pneumonic, may be absorbed sooner or later, and perfect recovery ensue.

There is a factor, upon which I would lay considerable stress, too often lost sight of in the discussion of this subject, which greatly aids us in explaining the occasional occurrence of consumption as the direct result of pulmonary hæmorrhage. This influence is the strumous or scrofulous diathesis. It is the absence or the presence of this condition of the system that explains the innocuous or serious character which bleeding of the lungs may assume.

In the cases detailed this factor existed, I believe, so far as can be judged by their history.

Many objectors to the *phthisis ab hæmoptoe* theory, will at once, I suppose, make a handle of this statement to claim the correctness of the doctrine laid down by Laennec, and may possibly cynically smile at him who may venture, in the temerity of youth, to differ from them entirely in this doctrine, while he asserts his belief in the strumous diathesis quite as strongly as the warmest supporters of such teaching. It is possible, I am persuaded, to meet a constitution in which inflammatory changes, under certain circumstances, will determine towards pulmonary consumption, while the lungs enjoy complete and perfect freedom from tubercular deposits prior to such inflammations and to such circumstances.

By the term "scrofulous" I would indicate that class of cases in which there is a tendency to inflammation and ulceration of the lymphatic glands, by rapid cell proliferation, in which by the rapidity of their formation, and the pressure thus necessarily exercised upon each other, a retrograde or cheesy metamorphosis ensues; be it seen in the enlarged glands in the neck, the strumous ophthalmia and eruptions of childhood, in the follicles and glands of the intestine and mesentery, or the lobular pneumonia of puberty—a class of subjects in whom inflammatory action is sub-acute, imperfect in its results, indolent in its progress, with feeble powers of repair, and in whom the blood may truly be termed "cacoplastic."

The recent experimental results attained by Drs. Burdon-Saunderson, Wilson-Fox, and others, at home and abroad, on the production and nature of artificial tubercle, seem to prove satisfactorily that the changes we designate tubercular—as contrasted with cheesy—begin in the adenoid tissue of the lung, and consist in the multiplication of perishable cells, which, losing their plastic or self-sustaining power as sarcophytes, rapidly retrograde into granular or fatty molecules.*

In the cases detailed I consider the phthisis was the result not alone of the acute catarrhal pneumonia, which had been set up by the blood in the alveoli, but most probably also of tubercles developed throughout the lungs consequent on the absorption of the blood effused and of the

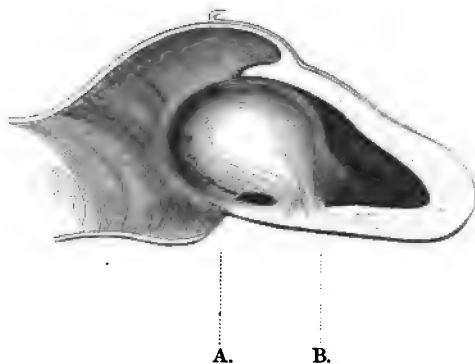
* Williams on Consumption.

products of such inflammation,—a pathological condition, which would seem borne out by the results of the experiments referred to. The facts brought out by these experiments, the logical conclusions deduced therefrom, and the strongly attesting evidence of experience, such as these cases and many other similar ones supply, satisfactorily explain to my mind the serious part pulmonary hæmorrhage may play in either producing consumption in previously healthy lungs, or, in lungs already diseased, in hastening the fatal termination, by lighting up afresh the dormant inflammation.

DR. M'SWINEY said whether as regarded the matter discussed or the manner in which it had been discussed by the learned author of the paper just read, it would be admitted that no more important paper had been presented to the Society. It was impossible to discuss it adequately during the short time that remained at their disposal that evening; and he therefore suggested that this important question be referred to an adjourned meeting of the Society, when an expression of the opinions of the practical physicians of Dublin could be given with respect to it. It appeared to him that there must be in Dublin an amount of experience in the matters that had been referred to so ably by the writer sufficient to give a well reasoned reply to many of the opinions he had propounded.

DR. GRIMSHAW seconded the motion, which was adopted, the Council to decide when the discussion should be resumed.

9 m. 10. 11.



A.—Anterior lip intimately attached to extremity of polypus.

B.—True pedicle.

The Polypus when removed weighed $\frac{1}{2}$ lb. Length while *in situ* 5 inches; but when removed and unfolded it measured $7\frac{1}{2}$ inches.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIFTH ANNUAL SESSION.

JOHN R. KIRKPATRICK, M.B., Honorary Secretary.

DR. ATTHILL, Vice-President, in the Chair.

Saturday, April 12th, 1873.

The VICE-PRESIDENT (Dr. Lombe Atthill) exhibited a tumour which he removed on the previous Thursday from a woman, a patient in the Adelaide Hospital. She was aged forty-two, was married, but never had any children. She enjoyed excellent health till two years ago, when she perceived that the menstrual periods were much more profuse than previously; then after a time they became more and more prolonged, and finally almost continuous. That was the only symptom she exhibited, except that at an early period she suffered from retention of urine for a short time. She suffered so much from loss of blood that she sought relief at the hospital. On examination a large tumour was found projecting into the vagina. Anteriorly, and to the right side this tumour could be traced up to the os uteri, to which it was attached so intimately as to be almost continuous with it. One could not make out any line of demarcation between the os and the tumour. Posteriorly the finger could not reach the pedicle; the sound passed in this direction nearly three inches above the point to which he could pass his finger. On Thursday, having had the advantage of the assistance of Dr. Kidd, and of Dr. Churchill, Dr. Atthill proceeded to remove this tumour. It was seized with a vulsellum and drawn downwards. He then passed a steel wire as high as he could posteriorly, but it soon snapped. He then tried a pianoforte wire which was annealed, and applied it in the same manner. That snapped also. A similar attempt with a very strong unannealed piano-string resulted in the breaking of the instrument. The resistance was something enormous. The question then arose whether the operation should not be postponed until a new instrument had been procured; but he did not like to defer the operation, and accordingly he proceeded to see what he could do with a pair of scissors. He could not reach the pedicle posteriorly; his efforts were therefore directed to the anterior part of the tumour. He with much difficulty cut through the attachment anteriorly, but was surprised to find the true pedicle was higher up, as represented in annexed woodcut. It appeared to him that at some time adhesive inflammation must have taken place, which had resulted in the intimate union of the tumour to the anterior lip. Having cut through the anterior attachment, he proceeded with a scalpel to divide the upper one, which

he succeeded in doing. Profuse hæmorrhage set in. Expecting hæmorrhage he had the actual cautery ready, and applied it freely to the bleeding surface. However, in a few minutes, blood coming again freely, he placed within the os uteri a pledget of cotton, saturated in a strong solution of perchloride of iron in glycerine—filled the vagina with cotton, and the bleeding ceased. The woman had a very feeble pulse when the operation was performed and she was now doing well. She was etherized by Mr. Morgan. Being in an anæmic state, with the pulse very feeble, his colleague Dr. Little advised him to give ether instead of chloroform. The operation continued an hour and twenty minutes, and during that time there was no struggle, no excitement, no sickness. So far as a single case went, he thought this case showed that ether was a very safe and good anæsthetic. The polypus would prove, he thought, an ordinary fibrous polypus. He was not acquainted with any case where an attachment had formed secondarily as in this case; but Dr. M'Clintock had told him he had seen some cases of the kind.

DR. CHURCHILL observed that this was a most interesting case. He had not the least hesitation in expressing his opinion that by far the best mode of removing a polypus that was outside the uterus and in the vagina, was by the vulsellum and a pair of scissors. There was no risk, no great hæmorrhage, and not the bother which attended the putting on of the écraseur.

The Excessive Vomiting of Pregnancy. By ALFRED H. M'CLINTOCK, M.D., &c.

"THERE is nothing in the whole range of physiology or pathology more extraordinary than the fact, that the gravid uterus, without itself being the seat of special pain, irritation, or disease, should excite fatal disorder by reflex irritation in some distant organ. In this way pregnant women may be destroyed by secondary disease of the brain, heart, lungs, kidneys, stomach, or intestines. In fact, there is, in particular cases, no limit to the poisonous influence exerted on the rest of the economy by the gravid uterus." Thus writes Dr. Tyler Smith,^a and his researches find no more striking illustration than the extreme and uncontrollable vomiting which is an occasional consequence of impregnation, and to which my present communication has reference. You will, therefore, understand that I do no more than allude to the ordinary sickness and vomiting incident to the gravid state, and which, though troublesome and distressing, are not in any way dangerous.

All cases of nausea and vomiting dependent on pregnancy might conveniently be disposed in three classes, according to the amount or degree of sickness present. In the *first* class sickness is confined to the forenoon,

^a *Obstet. Trans., Lond. I. 338.*

does not always or often end in vomiting, and generally passes away altogether at the period of quickening. In the *second* class vomiting is of more frequent occurrence, is not confined to the forenoon, and continues or commences to annoy the patient after she has passed the epoch of quickening. In the *third* group we may range those exceptional cases whose actual vomiting is so frequent and persistent that nutrition is arrested, wasting and debility rapidly ensue, and finally a febrile condition of the whole system, with symptoms of exhaustion, come on, resulting at last either in the death of the patient; or, in a very few instances, the cessation of vomiting, with or without spontaneous abortion.

Cases of this extreme kind are happily of rare occurrence. In the course of my experience in hospital and private practice I have met with only a very few instances where the symptoms were so urgent and so rebellious to treatment as to raise the question of inducing abortion, and in only one instance have I felt justified in resorting to this grave alternative. Very many cases, however, of this dangerous form have been recorded by English, French, German, and American obstetricians, so that it is a complication of pregnancy every accoucheur should be prepared to meet with. It may be present in first or subsequent pregnancies, but it is commonly supposed that women in their first pregnancies are more obnoxious to it, and the statistics I have collected give some confirmation to this opinion.

Another interesting question connected with excessive vomiting is the particular *period of pregnancy* at which it is most apt to occur. Of the recorded cases in which this circumstance is noted, the vomiting in the three months preceding quickening was of much more frequent occurrence than in the months after quickening. In a very few instances it began about one month after the supposed time of conception. M. Gueniot has given some attention to this point, and states,^a that of *forty-three* cases collected by him, the vomiting set in nine times in the first weeks of pregnancy; fifteen times at the end of the first month; nine times in second month; five times in third month; once in fourth month; twice in fifth month; twice in sixth month. In general terms, then, it may be stated that no pregnancy, and no period of pregnancy, except perhaps the first and the ninth months, is secure against the occurrence of this most formidable concomitant. It may recur in successive pregnancies. Thus Burns had to induce labour three times in one patient, on account of uncontrollable vomiting;^b and Mr. Garraway had to do it twice in a patient under his care.^c On the other hand, the patient on whom Dr. Munro operated passed through her succeeding pregnancy with only "slight nausea and sickness," which readily yielded

^a Quoted by Anquetin in *Rev. Medicale*, 1865, Vol. ii.

^b Midwifery, p. 265, tenth edition.

^c *Brit. Med. Jour.*, October, 1857.

to treatment;* and other cases of a like kind are reported. As regards the *etiology* of this excessive sympathetic disturbance, I fear it must be confessed we are as yet very much in the dark. Several explanations have been put forward, but none of them rests on any extended series of clinical or pathological facts, and can only apply to occasional instances. We may, however, regard it as pretty well established, that there are different morbid conditions, which, being superadded to the gravid state, may aggravate or excite the symptoms in question. These are, for example, congestive inflammation of the os and cervix; an irritable condition of the cervix uteri; ulceration of the os uteri; inflammation of the deciduæ; the effect of gravitation of the ovum "on some sensitive part near the cervix (Munro^b); or displacement of the womb.

Now, after making every allowance for the influence of these causes, there yet remains a large proportion of cases in which no evidence exists of any of them having been in operation, and for all such, the only explanation to be offered is that which attributes the sickness to over-distension of the uterine nerve fibres, and there are some clinical facts which apparently give support to this theory. Thus Robert Lee and Dubois have each related cases where the vomiting ceased immediately upon the discharge of the liquor amnii. In one case of Lee's the puncture of the membranes was not followed by any perceptible discharge of water; nevertheless, the vomiting "began immediately to subside, and she went to the full period, and was safely delivered of a living child" (Clin. Mid., p. 108, second edition). In another case (No. 68) related by the same observer, he tells us, "the vomiting ceased immediately after" (the puncture of membranes and discharge of the liquor amnii), "and the fever subsided, though the fœtus was not expelled for several weeks." A very striking case too was that of Dr. Campbell's (No. 35 in my table). Here a sound was passed into the uterus and the membranes ruptured. She vomited less that night, and next morning could retain food well; she improved from day to day, but was not delivered until twelve days after the operation. On the other hand, there are several instances where the gastric disturbance ceased upon the death of the fœtus only, and before any sign of abortion made its appearance; and again, in other cases where parturition was artificially induced, the vomiting persisted until the ovum was expelled, and these two series of facts are hardly reconcilable with the theory in question.

The flexion theory has lately found a warm advocate in Dr. Graily Hewitt. His paper was read before the Obstetrical Society of London, and is published in the thirteenth volume of its transactions. But, with the utmost respect for the author, I must candidly avow that, after a very attentive study of his essay, as well as of the discussion which followed

* Glasgow Med. Jour., August, 1872.

^b Op. cit., p. 6.

its reading before the Society, I could not find any facts or arguments to justify the conclusion that retroflexion or ante flexion of the gravid uterus is more than a very rare concurrent cause of the vomiting of pregnancy. The soundness of the theory is only to be tested by facts, but an appeal to such facts as are obtainable, brings out a mass of evidence which plainly forbids our accepting the statement that uterine malposition is "*the almost universal cause of the sickness of pregnancy.*" The one "typical case" related by Dr. Hewitt, gives no direct support to his views, unless we can suppose that the patient's being "much relieved" by confinement to the horizontal position, and attention to her bowels (which formed the whole treatment), furnish any grounds for the inference he would have us to draw from the case—viz., that the sickness was due to the ante flexion which co-existed with pregnancy.

I have met with several cases of retroversion of the gravid uterus, and in none of these cases was vomiting a prominent symptom, and in most of them it was entirely absent. In a considerable proportion of the recorded cases of excessive sickness, this (the sickness), as already stated, was present in the sixth, seventh, or eighth month of pregnancy, when ante or retroflexion was not only quite absent but the next thing to an impossibility. Dr. Barnes seems to think that the normal condition of the uterus in early pregnancy is one of slight anteversion, occasioning a change in the anterior wall of the vagina, which he regards as a valuable diagnostic mark of the gravid state.—(*Brit. Med. Jour.* 1868, Vol. ii., p. 204.)

Dr. Oldham has recorded a remarkable case in which the uterus was found retroflexed at the full term, and had probably been in this faulty position all through pregnancy;* and yet in this history there is no mention of sickness.

Dance, Dubois, Stoltz, and Kieller, have had opportunities of examining the bodies of women dying of the excessive vomiting of pregnancy, but in none of these autopsies was any displacement found to exist. Now, I do not want altogether to deny that displacement may give rise to the sickness, but merely to show it can only be a very rare and exceptional cause of it. Moreau and Briau had a few cases (three or four) in which the displacement of gravid uterus had, beyond doubt, very much to do with the vomiting, as this abated after the malposition was rectified. And in Dr. Munro's case, already cited, it is possible that the vomiting (which began so early as five weeks after the last menstruation) may in some degree have depended on the ante flexion; but it did not appear that there was any jamming of the fundus uteri, and the sound when introduced went upwards freely about five and a half inches. M. Stoltz relates a case he was called to, of this excessive vomiting in the third month of a first pregnancy, where he recognized the existence of retroversion of the uterus; and when the womb was replaced—and this could be readily

* *Obstet. Trans., Lond., I.* 317.

done—there was a temporary suspension of the sickness. As the rectification could not be permanently maintained, we cannot say whether, if such had been accomplished, the cessation of sickness would have lasted. Eventually abortion was induced as the only way of saving the life of the woman, and with a most successful result.

It is important to bear in mind that this vomiting, of which we are speaking, has no pathognomonic or specific character by which it may be recognized; its diagnosis must rest, therefore, on the co-existence of pregnancy, and the absence of any other cause for it, such as gastric, cerebral, hepatic, or renal disease. The purest cases, I believe, are those where it commences soon—i.e., a few weeks after conception; and its dating from this early stage may be regarded as a very strong evidence of the vomiting being a direct consequence of the gravid state. In several of the most marked cases the vomiting set in very soon after impregnation. Thus in Dr. Munro's case it began five weeks after the last menstruation. In the case to be hereafter related, it set in not later than the sixth week of pregnancy; and in Dubois' fifth fatal case sympathetic disturbance of the stomach was the very first symptom of pregnancy, appearing even before there was time to know if the menstruation was suppressed.

On the other hand I think there are good grounds for supposing that in many of the severe cases, where sickness commenced after quickening, it was not so exclusively due to direct sympathy with the uterus, but arose in part under the influence of some superadded complication. Thus the cases recorded by Dr. Hardy, and by Dr. Kidd (*Dub. Quar. Jour.* xxxviii., pp. 12 and 253), I would cite as illustrations of this statement. In each of them pregnancy was advanced to the seventh month, and in each there was present some other cause adequate to account for the prominent symptom. In one it was phthisis; in another gastritis apparently; and in the third constipation, which being removed the vomiting subsided. For the sake of diagnostic precision, therefore, we should distinguish between the sickness *of* pregnancy, and sickness *in* pregnancy.

The duration of the vomiting, before it brings the patient into a position of danger, is liable to a good deal of variation. In several instances this period has been under three or four weeks, and in others it was extended to eight, nine, or ten weeks. Of course very much will depend on the constitution and previous health of the patient, as well as the degree of intolerance shown by the stomach. An analysis of twenty-three fatal cases, by M. Gueniot, showed the mean duration of the disease to have been three months, which accords with the statement just made. The symptoms which are present in these extreme cases, are very well laid down by Dubois, whose description has been quoted by nearly every succeeding writer upon this subject. They are briefly these:—Excessive vomiting, all food, and sometimes even the smallest quantity of pure water, being rejected; emaciation and extreme debility, so that syncope takes

place under slight exertion, and obliges the patient to keep her bed; a febrile condition of the system, and an acid sour smell off the breath. Such a combination of symptoms would plainly indicate the patient to be in great peril, and, if medication has been judiciously tried without avail, it holds out only one mode of escape for the woman, and that is by terminating pregnancy, which has brought her into this all but moribund condition. If relief be not speedily given, and she be allowed to sink any lower, the time will have gone by for the intervention of art to save her life, and she will pass into that condition which Dubois calls the third stage, characterized by increased prostration, constant headache, impairment of vision, tendency to somnolence, and derangement of the intellectual faculties. To operate, he remarks, under these circumstances, would only bring obloquy on our art, and, perhaps, hasten the patient's end.

It has been urged by the opponents of artificial abortion, that we should leave it to nature to induce parturition in those severe cases where medicine fails to give relief; but this would be to abandon the patient to almost certain death, for the cases where in the advanced stage the vomiting has ceased, or spontaneous abortion has come on, are lamentably few: whilst the number of recorded cases where a fatal issue has taken place under these circumstances, or even after abortion, is very considerable. Dr. Munro, the latest writer on this subject, refers to twenty such fatalities. But with a very moderate amount of research I have been able to collect close on fifty authentically recorded cases, and I know of others which have not been published. Thus twenty fatal cases had come within the observation of Dubois; Churchill in like manner mentions four; Tyler Smith three; Stoltz three; Chaillay two; Dance two; and single cases are recorded by Murphy, Haighton, Lee, Maygrier, M. Hall, Breschet, Johnson, Danyau, Ulrich, Forget, Kieller, Lobstein, Caradec, Rigaud, Blot, Lancereaux, not including those cases where death took place after the occurrence of abortion.

From this sad array let us turn, and see what success has resulted from the timely interposition of art under the circumstances above described. When Paul Dubois and Danyau advocated this measure before the Académie Nationale de Médecine, in 1852, they could only adduce four or five cases in support of it, one of which cases occurred in the practice of Dubois himself, and was at the time a solitary success against three failures.

Since then the number of successful cases has greatly multiplied, that with very little trouble I was able to get the histories of thirty-authentic cases where this alternative measure was resorted to at every other mode of treatment had been tried in vain. Annetabular statement of these cases, showing, so far as data were given, the number of the pregnancy, the period of pregnancy at which the operation was, the result of the practice, with a reference to the name of the operator and of the publication where it is reported.

Cases of artificially induced Labour, on account of excessive Vomiting.

No.	Pregnancy	Period of Pregnancy	Result	Authority
1	Primipara	3rd month	Recovered	Munro—Glasg. Med. Jour., Aug., 1872.
2	—	7th month	Do.	Davis—Obstetric Medicine.
3	—	8th month	Do.	Ibid.
4	—	6th month	Do.	Ibid.
5	Multipara	6th month	Do.	Edwards—Ranking's Abs., iv.
6	Do.	3rd month	Do.	Churchill—Dis. of Women, 630.
7	Do.	3rd month	Died	Ibid.
8	Do.	8th month	Recovered	Copeman—Obstet. Trans., xiii.
9	—	8th month	Do.	Merriman—Med. Chir. Tr., iii.
10	—	—	Do.	Hergott.
11	—	—	Died	Aubenas.
12	—	—	Recovered	Burns—Midwifery, 265.
13	Multipara	—	Do.	Ibid.
14	Do.	—	Died	Ibid.
15	Do.	3rd month	Do.	Dubois—Bull. de l'Acad., xvii.
16	Primipara	1st month	Do.	Ibid.
17	—	2nd month	Do.	Ibid.
18	Primipara	1st and 2nd months	Recovered	Ibid.
19	—	3rd and 4th months	Do.	Harris—Phil. Med. Exam.
20	Multipara	6th and 7th months	Do.	Garraway—Brit. Med. Jour., Oct., 1857.
21	Do.	6th and 7th months	Died	Ibid.
22	—	2nd month	Recovered	Tarnier—Caseaux' Midwifery.
23	Primipara	2nd month	Do.	M'Clintock.
24	—	—	Do.	Trousseau—cited by Danyau.
25	—	4th month	Do.	Lee—Clin. Mid., 107.
26	—	2nd month	Died	Ibid.
27	—	6th month	Recovered	Ibid.
28	—	—	Do.	Ibid.
29	Multipara	8th month	Died	Ibid.
30	Do.	8th month	Recovered	Barnes—Lancet, 1863, Vol. i.
31	Do.	5th month	Do.	Griquet—cited by Danyau.
32	—	7th month	Do.	Cited by Danyau.
33	Primipara	3rd month	Do.	Stoltz—Gaz. Med., Juin, 1852.
34	Do.	7th month	Do.	Hardy—Dub. Quar. Jour., xxxviii.
35	Multipara	6th month	Do.	Campbell—Brit. Med. Jour., Oct., 1872.
36	Do.	2nd to 4th month	Do.	Dr. E. B. Sinclair.

The general result of these cases is simply this, that in *twenty-seven* instances the sickness was arrested, and the patients perfectly recovered, whilst in nine instances, although the vomiting ceased in nearly every one of them after the expulsion of the ovum, still the patients did not ultimately recover. Whether, or how far, the operation is chargeable for this unfavourable issue can only be estimated by a careful examination into the circumstances of each particular case, and the results of such examinations I now beg briefly to submit. In case No. 7, related by Churchill, the woman lived four or five days, and then died of diarrhœa, apparently brought on by over-feeding. In case No. 14, recorded by Burns, a biliary calculus was found impacted in the gall duct, which, no doubt, was the cause of the vomiting, and not pregnancy. Case No. 17 was in the practice of Dubois, who states that the woman survived until the sixteenth day after abortion, and then died of puerperal fever. No. 21 died on the tenth day after delivery, the cause being, as Mr. Garraway, who relates it, says, "sheer debility." This same patient suffered to such an extreme degree from sickness in her previous pregnancy, that Mr. Garraway had to provoke parturition as the only way of saving her life. She recovered, but her constitution was permanently and greatly weakened by the prolonged vomiting. No. 29 died of *post-partum* hæmorrhage. The case is recorded by Dr. Lee, having been seen by him in consultation. The woman was in the eighth month of her fifth pregnancy, and was greatly emaciated and reduced from long sickness of stomach and previous ill health. Consequently, she had no strength left to bear up against the effects of the loss. The instant the membranes were punctured the vomiting ceased.

In the remaining four fatal cases the operation was resorted to as a forlorn hope, but there were no reasonable grounds for expecting that it could succeed, to such a deplorable state of exhaustion were the patients reduced by the prolonged and incessant sickness of stomach.

The result then of this analysis of the nine fatal cases is no way disparaging to the operation, as in five of them the unfavourable issue was attributable to the fact of the interposition of art having been too long delayed (as will happen with any operation, however good in itself, and however skilfully performed); whilst in the remaining four cases the cause of death was purely accidental, and not directly referable to the operation.

Cases will occasionally be met with where, along with the vomiting, are symptoms which would lead us strongly to suspect the existence of actual disease of the stomach, liver, kidneys, or some other organ. Here a grave question will arise, viz., whether the vomiting be dependent on pregnancy at all or not. These are a very perplexing class of cases, and require the practitioner to use the utmost caution and discrimination before deciding on the expediency of inducing labour. In cases of this

description one would be inclined, *primâ facie*, to pronounce against its adoption, lest the vomiting might continue; in spite of the uterus being emptied (as I have known to happen), which would, of course, expose the operator to censure, unless in his prognosis he had fully anticipated the possibility of such a contingency, and prepared the friends for it.

In most, if not in all, of these complicated cases, however, the gravid state is itself an important factor of the vomiting, so that cutting short the pregnancy will often tend to remove or most materially alleviate the symptom which menaces the life of the patient. Examples of this are to be found among the cases in the table. Thus, in Dr. Hardy's patient (No. 84) dark fluid was ejected from the stomach, with blood in large quantity, and the burning sensation was intolerable. In the case recorded by Dr. Barnes (No. 30) there were hæmatemesis and scirrhus-like induration of the cervix, which was nodulated; and in my case slight jaundice was present.

Dr. Churchill has favoured me with the notes of a case bearing very closely on this part of our subject, and which I, therefore, make no apology for introducing. He writes:—"I was called to Mrs. W. early in November, and was told that she was between three and four months pregnant of her fifth child. She had always suffered from sickness in her former pregnancies, but never to such an extent as now. Whether she took anything or nothing she was incessantly retching and vomiting. Not a particle of anything rested on her stomach. I found her looking very ill and exhausted, with a pulse of 120 and *dry tongue*. There was no pain on pressure in any part of the abdomen, but I found a considerable enlargement of the liver, with a very weak heart. I tried the usual remedies with some slight alleviation, but it was clear that unless some more decided relief was obtained she would sink. I, therefore, asked for a consultation, and Dr. Denham agreed with me that the induction of abortion could not be long deferred, but that as there had been less vomiting during the day (we met in the evening), we might wait until morning, meantime freeing the bowels.

"In the morning I found that a foetus of near four months, alive, had escaped from the uterus, without any pain or bleeding. The after-birth was retained, but as there was no discharge I determined to let her rest. The vomiting *scarcely* returned after the abortion, but the sinking *increased*, in spite of all our remedies, and she died on the third day after the expulsion of the foetus, and the seventh from my first visit."

That the vomiting in this case was dependent mainly upon the pregnancy is proved, I think, by its cessation as soon as the foetus had come away. The death of the patient some hours subsequently well exemplifies the observation of Dubois, that if the symptoms go beyond a certain limit, before abortion occur, this will be productive of no real benefit.

Having given this brief clinical retrospect of the subject before us, let

me now submit a concise history of a case which lately fell under my own notice.

Mrs. —, aged twenty-two, of robust and active habits, was married 5th June, and conception took place it was supposed between the 14th and 20th. Her health continued uninterruptedly good up to 20th July, when vomiting set in rather suddenly, and recurred with increasing frequency, and with daily declension of her strength up to the time of my seeing her on 19th August, in consultation with Dr. Murray, of Delvin, and Dr. Ridgway, of Oldcastle. She was then, and for some days previously, altogether confined to bed, from sheer weakness. The attempt to sit up brought on retching and faintness, and from having been moderately stout she had become wretchedly thin. Her face was flushed, and she was slightly jaundiced. The tongue was clean, and the pulse seldom above 80; the belly was flat, rather retracted indeed, and free of tenderness; the bowels, generally inclined to constipation, had been well opened by medicine a few days ago, and there was no evidence of any lodgment in the colon. The urine was sufficient, of high colour—from bile apparently—and free from albumen; there was no dysuria or irritability of bladder, nor had there been. The stomach rejected almost everything she took, whether fluid or solid; there was no one thing that appeared to be tolerated, although the entire quantity of everything swallowed might not be vomited. Glairy mucus, or yellow bile sometimes came up with the food and drinks. Her nights were pretty good, though her sleep was much broken and disturbed.

The uterine tumour could not be distinguished above the pubes; but, *per vaginam*, the body of the organ was felt enlarged and slightly anteverted, as is often found to be the case at this period of utero-gestation. The os and cervix were healthy to the touch and to the eye, and free of any undue sensibility, heat, or fulness. I fully coincided in the opinion that she was pregnant, and that the vomiting was sympathetic of this state. Nearly all the remedies and modes of treatment usually employed in cases of this description had been tried, but with no other result than a very occasional and brief mitigation of the sickness. Although it did not seem possible she could exist much longer, unless the sickness was very decidedly relieved, still we thought the symptoms did not present that degree of pressing urgency to justify our resorting to so extreme a measure as the induction of abortion. The following line of treatment, therefore, was agreed on:—A small blister over epigastrium, half a grain of calomel and two grains of dried soda in pill every six hours, hypodermic injection of morphia, daily inunction of the trunk with olive oil, nutritive enemata of beef-tea or milk every six hours; diet to consist of very small quantities of lime water and milk, beef-tea, cold chicken jelly, brandy, and saltzer water.

Ten days after this, namely, on 29th August, I saw this lady again, as the persistence of the vomiting, the accession of pyrexial symptoms, and the extreme prostration of strength, too plainly showed that unless relief was obtained she could not survive many hours. On this occasion I met Dr. Ridgway only, as Dr. Murray was not able to be present.

A very marked change had come over her since my former visit—ten days before. Her face was contracted and of a dusky red colour, and wore an expression of intense weakness; the eyes looked heavy, and the conjunctivæ were slightly yellow. She complained that her sight had become impaired; her voice was much altered and was weak and rather husky. Even with assistance she was scarcely able to sit up, and to get her out of bed produced alarming syncope. The pulse was thready and weak, varying in frequency from 100 to 120 or 130. The skin was hot and dry; tongue clean, but very red at the edges; slight mercurial ptyalism was present. It was with reluctance she took food, as it was so sure to be rejected, and the act of vomiting produced intense depression.

We were now quite agreed that the time for putting in practice the only remaining alternative—viz., artificial abortion—had arrived, and to postpone it any longer would be to deprive the patient of all chance of benefit from its employment. Indeed, to be candid, I had serious misgivings that the interposition of art had come too late. We explained to her husband and friends what was going to be done, fully stating the possible dangers of the operation; and, having obtained their consent to its performance, we at once set about it. She was so lamentably feeble that we had to proceed very slowly and with the utmost gentleness. The sound was passed with great ease into the uterine cavity to the extent of four inches, and moved freely about, but no water came away. Towards the fundus it encountered some abrupt inequality, which I concluded was the embryo. A small whalebone stilette was next passed up to same extent, and with same negative result. A laminaria bougie, No. 8 size, was now introduced to the extent of three and a half inches, and left in utero—a small sponge being placed in the vagina to prevent the tent coming away. These manipulations occupied some minutes, and seemed to distress the patient a good deal. It was agreed she should get an enema with two drachms of Long's liquor ergotæ, in the course of some hours, should pains not supervene. For the subsequent history of the case I am indebted to the kindness of the medical attendants before-named.

For the next twenty-six hours the sickness continued apparently with greater severity, and her weakness was extreme. Uterine pains now came on, and in four or five hours some hæmorrhage having appeared, the sponge and tent were removed, having been about twenty-nine hours *in situ*; soon afterwards Dr. Murray was able to take away the ovum; the embryo was one inch and a half long, and healthy in appearance. Though the hæmorrhage had been moderate and was now over, she fell into an

unconscious state, resembling syncope, which seemed to be mortal. This lasted upwards of an hour, when she was at length aroused by the unremitting exertions of the doctor and nurse. During this trying period, more than once it was thought that life had fled. By the steady employment of restorative measures for some hours she gradually and completely revived. From this time the irritability of stomach began to subside, but it did not entirely cease for some days, as vomiting occasionally took place when she was not careful to observe our injunction to eat and drink in great moderation. With these trivial exceptions her convalescence proceeded most favourably, insomuch that she was able to sit up for a short time at the end of a fortnight, and this improvement went on steadily until her health and strength were completely re-established.

The VICE-PRESIDENT (Dr. Atthill) said the subject treated of by Dr. M'Clintock was one of great importance. He had shown very properly that the sickness of pregnancy was twofold, "normal" and "abnormal." He distinguished the sickness of pregnancy from the sickness that may occur during pregnancy. The latter depended on a variety of causes, constitutional or otherwise, such as consumption, gall stones, &c. With these special causes Dr. M'Clintock did not deal. What the Society had to consider that evening was the sickness of pregnancy. With regard to the flexion theory to which Dr. M'Clintock had referred he could not understand how it could be entertained by any well-informed medical man. They must all have seen cases of well-marked flexion of the uterus where pregnancy occurred, and no vomiting followed. He (Dr. Atthill) had a patient whose uterus was always distinctly retroflected. When pregnancy occurred the uterus still lay into the hollow of the sacrum. This patient did not suffer at all from vomiting. Some time ago a lady consulted him for menorrhagia, which he found to depend on an imperfectly involuted uterus which was retroflected. He told her she must submit to treatment, but she had to leave town and a delay of two months occurred. On her return he found she was pregnant. The uterus was still completely retroflected, no treatment was adopted, the case went on to the full term, and there was no vomiting at all. That case, to his mind, clearly disposed of the flexion theory. He looked on the vomiting of pregnancy as a useful symptom, except when it occurred in an extreme degree. Its cause, however, was not, and probably never would be, determined accurately. He was inclined to think it might be due to the distension of the *os internum*. He had seen nausea produced by the passage of the uterine sound through a narrow *os internum*; it also occurred in those cases of dysmenorrhœa, which depended on the formation of clots in the uterus; when these clots were passing through the *os internum* vomiting often occurred. After the passage of a clot the vomiting would cease and would not recur. He thought, therefore, the cause of vomiting

in some cases might depend on the stretching of the os internum. Of course the over-distension of the nerves of the uterus might be another cause, as Dr. M'Clintock had observed. The treatment of procuring abortion ought to be adopted in extreme cases to prevent sinking. They should not hesitate to sacrifice the foetus when the mother's life was in danger, for they might sacrifice both the foetus and the mother's life if they did not interfere.

DR. CHURCHILL said it was an extremely important subject, a very grave subject, and one on which it was difficult, as to treatment, to make up one's mind—he meant more as to the time of interference than as to the fact of interference. First, with regard to the retroflexion theory, he thought it could be settled in a very simple way. Without having had an unusually large practice, he had seen more fatal cases of vomiting in pregnancy, in consultation and otherwise, than he had ever seen of retroverted uterus during pregnancy. A retroverted pregnant uterus was not an everyday occurrence; whereas sickness, and very sharp sickness too, was of ordinary occurrence. Now, if a retroverted uterus was the cause of the severe vomiting of pregnancy, it ought to be a very frequent occurrence; whereas, in his experience at least, a retroverted pregnant uterus was not a common occurrence. He thought that ought to settle the question. No doubt there were some states of the cervix uteri that give rise to this excessive vomiting. He had seen the most typical case of morning sickness of pregnancy where the lady was not pregnant at all. She would be sick on getting out of bed; then the sickness would subside and she would go down to breakfast; and when he applied something to the cervix uteri she would be perfectly free from the vomiting for a certain time, after which he had to repeat the application. He believed that various conditions of the os uteri, of the cervical canal, and of the os internum might be a cause of this sickness. But to come to the question more immediately the subject of Dr. M'Clintock's paper—that is, the extreme vomiting of pregnancy—he had seen altogether seven cases, of which five turned out to be fatal. Some of them were seen by him in consultation when the woman was at the last gasp. In only one of these cases was the child alive, and that was the case where nature had taken the case into her own hands, and had failed as signally as if he had done it himself. These cases all occurred in an early period of pregnancy, and, as a general rule, the suffering was that of exhaustion; but in one of those cases the expression of human agony could not have been more intense. In that case he succeeded in saving the patient's life. He put in a sponge tent, and the foetus was expelled, and the woman was still alive. The great difficulty was the time at which the operation was to be performed. He thought they might dismiss any consideration as to the foetus. A good

many of them were dead before even the question of operative interference arose; but if they could not save the child and could save the mother, he held that their duty was clear. He thought one of the best guides as to the operation was the pulse. In Dr. M'Clintock's case, so long as the pulse was not 80, he did not interfere. In all the bad cases he had seen, and some of them were reported, the pulse became very high; and he thought it would be a wise thing to take operation into immediate consideration when the pulse rises. With regard to the last of his cases, which had been referred to, the operation was deferred too long. He proposed it, but there were divers difficulties in the way; and as the vomiting was not increased, he allowed himself to be overruled. In this he was wrong; for when a man felt he was right he should not allow himself to be overruled in such a matter; but the case resulted fatally, and he thought the mistake made was in postponing the operation too long. If the patient were allowed to run down to a certain extent she could not rally, and the really difficult thing was to hit upon the point when she was not too far gone for interference, and to know when to avoid interfering unnecessarily. He was inclined to take her general condition into account, but to have regard especially to the pulse. In the case of his, already referred to, there was an additional difficulty—a very enlarged liver; and they could not be sure that the vomiting did not depend on that; and one of the doctors engaged in the consultation held that opinion very strongly, because he thought the premature labour ought to have been brought on twenty-four or thirty hours before it occurred. He could state most positively that in all of the seven cases to which he had referred, there was no flexion of any kind.

DR. BYRNE had seen a great number of dangerous cases of vomiting in pregnancy, but had seen only one fatal case, and that occurred many years ago when he was a very young man, and before he became engaged in special practice. He was called to see a young woman who had been five or six months married. She had been delicate, and soon after marriage she was attacked with vomiting, and at the period he saw her she was four and a half months pregnant. Every day her condition became worse; her pulse rapidly got up; nothing would remain on her stomach, and eventually the matter she rejected from her stomach assumed the appearance of green vomit. He called in an eminent physician, and they determined to see her the next day before trying any decisive measure. During the night she got symptoms of labour, and miscarried of a small four months foetus. She immediately sank. His own impression was that she was not phthisical, although a delicate woman. He had seen a great number of cases where there was no approach to fatality, but where death would certainly have resulted if

the symptoms had not yielded to treatment. As to the cause of excessive vomiting, he thought they were in the dark. He did not think it depended on an alteration in the position of the uterus. He had seen cases where pregnancy came on in a retroflexed uterus. A few years ago he was called to see a lady four months pregnant, and she was suffering from retention of urine; the fundus of the uterus was completely down in the hollow of the sacrum; it afterwards went up of itself, and during the whole time she had not the least symptom of vomiting. In most of these cases of great sickness of the stomach the uterus was found in its normal position, that is, slightly ante flexed. It was possible, however, that an alteration in position might be one of the factors that produced vomiting. Dr. Atthill threw out a suggestion that sickness might be produced by distention of the internal os, as in the case of the passing of clots of blood through the os internum. He (Dr. Byrne) could not agree in this view, because if the internal os were distended it would be a preparation for labour or the passing of the ovum, because the os could not be stretched without allowing part of the ovum to pass through it. During pregnancy the whole of the cervical canal was closed, and he thought the illustration of passing a sound through and irritating the internal os would hardly apply. His own impression was that it was the stretching of the fibres of the uterus that caused the sickness. When they considered the small size of the uterus in its unimpregnated state, and that when impregnation occurred it received a very large supply of blood, and had to accommodate itself to the rapid growth of the ovum, it is not unreasonable to suppose that during the first pregnancy some alteration would take place leading to vomiting. It was remarkable that where vomiting set in pregnancy went on better, and it was desirable, as a general rule, to see a little sickness in these cases. With regard to the operation of inducing premature labour, they should not decide on it without the most mature consideration. If the doctrine were generally preached that artificial labour might be brought on, it might probably be resorted to in many cases where the sickness would have ceased if left to itself. The suddenness with which it sometimes ceased was very remarkable. He had seen cases where one would have thought the patient was going to expire, and the very next day she would be perfectly well, and enjoy good health during the rest of her pregnancy. They should, therefore, try the effect of every known remedy and change of air, if possible, before resorting to the operation of producing premature labour. He had seen a good deal of relief derived from the exhibition of the salts of cerium, and he had also seen two or three cases where sub-cutaneous injection had been attended with benefit. He repeated, therefore, that they should be cautious in recommending the production of premature labour, first, because that line of practice might be adopted in cases where it was not necessary; and,

secondly, from the sudden manner in which they saw a cessation of this sickness.

The VICE-PRESIDENT (Dr. Atthill) said, in explanation, he looked upon the vomiting of pregnancy as a healthy occurrence, because it facilitated the relaxation of the os internum. He believed it was an error to think that the os internum was closed during the early months of pregnancy. The over-extension theory might be right in some cases where there was a rapid and abnormally great accumulation of liquor amnii; but it must be borne in mind that the uterine wall increased by growth and not by extension, and in general over-distention was not the cause of the sickness.

DR. DARBY said every one had met with cases of moderate sickness, which was not a very alarming symptom in the early stages of pregnancy, and he was disposed to attribute it to a peculiar idiosyncrasy unconnected with any organic affection. As to retroflexion of the uterus being the cause, he was altogether an unbeliever in it as far as his experience went. With regard to organic affections complicating pregnancy, he had met with some cases of the kind, and they were very serious. The worst case he ever met with was that of a lady in her first pregnancy. She had jaundice, and the sickness and incapacity of taking food were greater than he had ever seen before or since. He had it in contemplation whether he should not interfere and procure abortion. Before he did this he requested the assistance of Dr. Churchill, and when he saw the lady he advised him to wait, and he did so, and the patient recovered. He believed that the greater number of the milder cases of the sickness of pregnancy might be attributed to idiosyncrasy, and not to organic change.

DR. BYRNE wished to explain that he did not mean to convey that the uterus was merely stretched by the growth of the fœtus. They all knew that the walls of the uterus increased in size by a process of growth in the uterine tissue itself, as well as increased in circumference.

DR. M'CLINTOCK was glad to see that the experience and reflection of the gentlemen who had addressed the meeting confirmed and emphatically endorsed his opinion with regard to the flexion theory, which he would scarcely have thought it worth while to notice, except that it came from a man of such a high position as Dr. Graily Hewitt, an ex-President of the Obstetrical Society of London, and whose name was so well-known as an author. The first great point of practical importance in the cases of excessive vomiting was to make a correct diagnosis, and ascertain beyond all doubt that the sickness really depended on pregnancy and not on any extraneous cause. Many cases would occur where this question was

surrounded by difficulties that would tax to the uttermost the skill and discrimination of the medical man. That question being cleared up, the next great practical question which occurred was to know the exact time to interfere. How long may we trust to medical treatment, or keep on temporizing in the hope that a favourable change may take place or nature interfere? Two cases were narrated by the speakers that evening which carried with them great weight—namely, that in the last extremity of sickness nature did interfere and caused the expulsion of the ovum, but it came too late; in fact, this expulsive action of the uterus took place because the patient was about to die.

The Society then adjourned.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

DR. KIDD, President.

Fibro-Sarcomatous Tumour of the Superior Maxilla.—MR. WILLIAM STOKES exhibited a tumour which he had recently removed from the side of the face of a patient under his care in the Richmond Hospital. It was of eighteen years' growth. The patient was fifty-eight years of age, and had always lived a temperate and regular life. He stated that eighteen years previous to admission to hospital, an unsuccessful attempt was made by a local dental surgeon to extract a tooth, and that the tooth broke. Three or four months after that, he observed a small tumour, of the size of a hazel nut, which appeared behind the situation of the tooth. This was soft, and on squeezing it externally some serous matter was pressed into the mouth. This was followed by a diminution in the size of the tumour, but it filled up again, and so it went on slowly increasing till it reached the size of a bantam's egg. It then went on rapidly growing, and in the course of a year he presented himself at the hospital. On examining the tumour it was found to be uniformly soft and elastic, free from pain, and the skin was perfectly free and movable over it. There were no signs whatever of glandular contamination, and about the centre of the tumour there was an ulceration which had none of the characteristics of the ulcers seen in connexion with carcinomatous affections; the edges were smooth, there was no fœtor from the discharge, and the surface was covered with healthy granulations. The ulceration was evidently caused by the pressure of the tumour on the integuments. The patient was free from lancinating pains, his appetite was good, he slept well, and had not got that peculiar cachectic appearance that people generally have who are afflicted with cancer. Looking at all these circumstances—the great chronicity of the case, the absence of glandular contamination, the appearance of the ulcer, the health of the patient being generally unaffected, he came to the conclusion that the tumour he had to deal with was not a cancerous one. He accordingly recommended an operation for the removal of this growth. The removal of it along with the greater portion of the superior maxillary bone, was a proceeding unattended with any great difficulty. There was smart hæmorrhage, which was controlled by a free application of the actual cautery, and which he used also to destroy any portions of the diseased tissue which might have remained. Nothing untoward occurred during the convalescence of the patient, and the wound healed rapidly, except at two points—at one of which there was a slight opening; but a trivial plastic operation would amply suffice to fill that in. Anxious that the

view he had taken of the tumour should be confirmed or otherwise, he asked his colleagues, Drs. Harvey and Yeo, to examine it for him microscopically, and it was satisfactory to him to find that the result of their examination confirmed his opinion that the tumour was not of a malignant nature.—*December 14, 1872.*

Disease of the Pulmonary Arteries ; Hypertrophy of the Right Ventricle.—

DR. YEO said the morbid specimens, which he laid before the Society, were removed from the body of a female, who had been repeatedly under observation during the past seven years.

She was first admitted into the Whitworth Hospital, suffering from acute rheumatism, complicated with endocarditis. She left hospital cured of the rheumatism, but with some traces of the endocardial mischief remaining. Soon afterwards she was re-admitted with well-marked signs of narrowing of the mitral orifice, which was soon followed by pulmonary engorgement. Since that time she has frequently been in hospital, suffering from the same train of symptoms, namely, dyspnoea, cough, hæmoptysis, and palpitation of the heart. For the last year she has been almost constantly under observation. During this time the posterior and inferior portions of the chest were usually dull, the anterior and superior parts resonant. Over the dull parts crepitus and muco-crepitus were audible, while over the resonant regions, loud sibilant râles were usually heard. The area of precordial dulness was increased, and daily became more and more extensive. A loud systolic bruit was audible at the apex, and a soft double murmur at the base of the heart. The liver was considerably enlarged for the last twelve months. The lower extremities of late became œdematous, to which was added a variable, but considerable, amount of ascites. On three different occasions she suffered from symptoms which were attributed to the impaction of emboli in the systemic vessels—viz., considerable pain and swelling in a limb, and the pulse in the artery suddenly becoming imperceptible; these symptoms usually subsided in a few days, and the circulation was re-established. During the last couple of months of her life the action of the heart had been extremely irregular and rapid, so much so that the sounds could hardly be differentiated except when she was more or less under the influence of digitalis. Four days before her death the dyspnoea suddenly became more intense and she rapidly sank.

The *post-mortem* examination revealed the following interesting pathological changes:—On opening the pericardium the right ventricle only was visible; it can still be seen to form the greater part of the heart's surface. It is dilated and distended with dark semi-coagulated blood; its wall stands prominently forward and does not collapse; it is enormously hypertrophied—in one part measuring three-fourths of an inch, and in most places exceeding in thickness that of the left ventricle; the pulmonary

semilunar valves are quite healthy; tricuspid is slightly thickened. The right auricle was found distended with dark fluid blood, and its appendix tightly packed with irregular masses of greyish-brown, crumbling fibrin. The left auricle is small, its wall slightly thickened; its appendix also contains a considerable quantity of old fibrinous clots. The left auriculo-ventricular opening is of cartilaginous hardness, and so narrow as not to admit the tip of my little finger to pass through it. The mitral valves are thick and extremely contracted; the cordæ tendinæ have quite disappeared—the muscoli papillares being attached directly to the margin of the thickened valves. The left ventricle is somewhat hypertrophied; the aortic semilunar valves rigid—two of them grown together as far as the centre of their margins. The aorta is quite healthy.

The lungs are dense, airless, and œdematous throughout, except at the anterior and inferior margins, where there is some emphysema, and posteriorly where they are engorged. A clear, sanious fluid, almost quite free from froth, exudes from their cut surface. On the anterior aspect of the upper lobe of the left lung is a dense dark prominent swelling about the size of a walnut. At the posterior edge of the lower lobe is a similar hard, hæmorrhagic prominence about the size of an orange. On the anterior surface of the lower lobe of the right lung are two other hard, black swellings, the upper one about the size of a cherry, and the lower, which extends over the diaphragmatic surface, is about the size of a hen's egg. All these tumours are distinctly circumscribed, and when a section of them is made, they appear pyramidal in shape, their apices pointing towards the root of the lung, and their bases corresponding to the pleura. The cut surfaces exude a dark blackish fluid, the two larger being very friable.

The pulmonary artery, from the semilunar valves down to the very finest subdivision, is irregularly dilated and inelastic, the inner coat throughout being studded with hard prominent yellow patches, some of which are rough on the surface, and look like ordinary atheromatous ulcers. The disease seems to have attained its maximum in the smaller branches, but was also present, though to a less degree, in the immediate vicinity of the heart. The vessels supplying the hæmorrhagic swellings, which enter at their apices, are found to be filled with tough, laminated, fibrinous plugs, which adhere firmly to the walls of the vessels.

The liver was very large and intensely engorged. The kidneys showed some signs of parenchymatous nephritis; the other organs were apparently healthy.

Dr. Yeo considered the case instructive on account of the following considerations:—Such a considerable amount of so-called atheromatous degeneration, as was present in this case, is seldom met with in the pulmonary artery. Indeed, Rokitsansky,* explaining this pathological

* Ueber einige d. wichtigsten Krankh. d. Arterien, 1852.

change in accordance with his "humoral" views, attributed its causation to the kind of blood contained in the vessels, asserting that only arterial blood was able to produce the affection process, and he thus denies the possibility of its occurrence in the pulmonary artery, while he admitted it in the veins. The exact opposite has been asserted by Dittrich^a who has devoted considerable attention to this subject. Virchow,^b too, regards it as a recognized fact, that the pulmonary artery invariably suffers more severely and earlier than the pulmonary veins in valvular disease of the heart, and this he explains by the increased tension, and consequent mechanical irritation kept up in the pulmonary artery by the engorgement of the lung. For he considers this disease of the vessels to be a form of chronic inflammation, caused by prolonged mechanical irritation, and he suggests the name, *Endarteriitis Deformans*, as preferable to atheroma, which is only one of its many results.

Prolonged impediment to the pulmonary circulation is a common occurrence, but very great increase of strength of the right side of the heart is rare. In this case it is most remarkable,—the cavity of the right is nearly double as large as that of the left ventricle, while its wall is about the same thickness. The occurrence of two rare lesions developed to such an extreme degree in the same individual, leads me to the idea that the dilated and inelastic pulmonary artery affords a ready and satisfactory explanation of the cause of the excessive hypertrophy of the right ventricle; just as a diseased aorta produces hypertrophy of the left ventricle.

The hemorrhagic swellings in the lungs are good examples of *Infarctions*, as these pulmonary apoplexies are commonly called, since Virchow,^c in 1846, explained the mechanism by which embolism produces intense engorgement of the territory supplied by the plugged vessel, in those tissues which have only a capillary anastomosis. The friable fibrinous clots in the auricular appendix, furnished a very accessible source for the emboli found in the arteries leading to the infarctions. In some of the cases which Laennec^d so accurately details in his original description of pulmonary apoplexy, the origin of the emboli was in all probability the same as in this case. The impaction of the emboli probably took place at different times within a few days before her death, causing the sudden and intense dyspnoea. Thus death was caused by the lesion at the right side, while the endocarditis of the left side was evidently the remote cause of all the changes.

The chronological order of the various pathological events may thus be briefly enunciated:—The acute rheumatic endocarditis resulted in aortic

^a Ueber d. Lungen-Infarctus, 1850.

^b Gesammelte Abhandlungen, p. 506.

^c Traube's Beiträge Zur Experimentellen Pathologie u Physiologie. Berl. 1846.

Hft. II.

^d De l'Auscultation Médiate: Paris. 1819. Tom. II. Obs. xxxv.

insufficiency and intense mitral stenosis. These heart complications naturally produced engorgement of the lungs and increased tension within the pulmonary arteries. This habitual distension of the vessel produced constant irritation of its coats, and thus excited extensive chronic *endarteritis deformans*. The right ventricle, having to overcome the difficulty presented to it by the artery in addition to the impediment in the lungs, became thus enormously enlarged. The same impediments to the circulation caused dilatation of the auricle, retardation of the blood, and thrombosis of its appendix. Particles of this thrombus, broken off and carried into the pulmonary artery, were impacted in its branches, and these emboli gave rise to the hemorrhagic infarctions which we have seen.—*December 14, 1872.*

Fracture of the Neck of the Femur.—DR. WHARTON exhibited a recent specimen of fracture of the neck of the femur, which occurred under the following circumstances :—The subject of the accident, which happened on the 24th of the last month, was an elderly female, aged sixty-five. She formed one of a crowd of recipients of coal tickets which had assembled outside the office of “the Sick and Indigent Room-keepers’ Society,” in Palace-street. In her desire to make progress through the crowd she received, as is alleged, a push from a constable, and was thrown down on her side. Having thus become disabled she was brought to the Meath Hospital, where she was placed under my care. She was an exceedingly spare woman, and although the femur presented the appearances of a well-developed bone, yet she was so emaciated that her limbs seemed as if covered with skin only. Her spinal column was deformed to a remarkable extent, the left scapula was excessively protruded, and the sternum arched to a corresponding extent. She could only rest in one position, and required a suitable disposition of pillows and bolsters, in order to compensate for the deformity under which she laboured. On examination of the left hip, to which she referred as the seat of pain, there were no marks of violence to be observed, nor did she complain of any ill results from the handling of the part. Upon comparing the length of the affected limb with its fellow, it was found that the former was scarcely half an inch shorter, and the foot of the same side was moderately everted. On being asked to raise the limb from the bed she was unable to do so, but could flex the knee slightly. In a week after her admission she died unexpectedly, and on the following morning a *post-mortem* examination revealed the condition of the parts now exhibited. During the dissection, and before the capsular ligament had been reflected, it was imagined the fracture was partial, in consequence of the very small transverse rent which appeared in this structure. In this, however, he was deceived, for on bringing the neck and head of the femur into view, which were nearly at right angles with the shaft,

it was found that the fracture was complete. The fracture was thus intracapsular and not impacted, in consequence of the integrity of the cervical ligament at the posterior part of the neck. The absence of any very appreciable shortening of the limb was thus accounted for. The signs and symptoms, as well as the *post-mortem* examination of this case, confirm the description of this accident, as laid down by Professor R. W. Smith, who is justly regarded as pre-eminently the authority on the subject. A reference to his treatise on Fractures and Dislocations, at p. 44, where he brings under review the doctrine of "partial fracture of the neck of the femur," a subject which had engaged the attention of Mr. Robert Adams and the late Professor Abraham Collis, will amply repay those who are interested in the matter under consideration, and specially so when taken in connexion with the lesion now exhibited.—*January 11, 1873.*

Cerebral Aneurism.—DR. GRIMSHAW exhibited an example of aneurism of the basilar artery, which had caused the death of the patient from apoplexy, by rupture of the sac. The patient was admitted to Steevens' Hospital on the 23rd December. Up to that day there had been no symptoms of apoplexy, or anything resembling it, or of paralysis. On the morning of his admission he went to a privy at the rear of the house he lived in, and while there he suddenly became insensible, and fell down in an apoplectic seizure. One of the demonstrators at the hospital, Dr. Tweedy, went up to the place, and found the man suffering from apoplexy. He (Dr. Grimshaw) did not see him on that day, but on the following morning he found him almost insensible. He could pinch him with impunity and touch the eyeball without producing any sign of sensibility in the conjunctiva. The right pupil was normal, and the left dilated, but both were insensible to light. The breathing was abdominal, and of an ascending and descending character. There were ten respirations in as many seconds, then a cessation of breathing for fifteen seconds, followed by another quick breathing of ten seconds' duration. The patient passed water after his admission to hospital. A catheter was passed, and drew off a small quantity of urine. Dr. Grimshaw concluded that he had to deal with an apoplectic effusion of blood at the base of the brain. The man died on the following day without any convulsion. Dr. Grimshaw then exhibited the aneurismal sac, which was situated on the left side of the basilar artery. The whole of the brain was covered with effused blood; it found its way into all the fissures in the neighbourhood, and it had also penetrated into the ventricles, there being a clot in the posterior corner of the right and anterior corner of the left lateral ventricle. In the fourth ventricle, also, there was a coagulum. This appeared to be the commonest form of intracranial aneurism. It was mentioned as such by Dr. Smith in his communication to this Society, and any statistics furnished since confirmed

this view. Whether this aneurism was caused by embolism or not it was difficult to say. The heart was not examined, but during life Dr. Grimshaw found no sign of disease. No obstruction of any kind was found in the neighbourhood of the aneurism. All the arteries at the base of the brain were healthy; the membranes of the brain were all examined for miliary aneurism by Dr. Bookey, who made the *post-mortem* examination, and carefully investigated every part of the specimen, but none was found. As to whether this was or was not a true aneurism, we made as careful an examination as possible. It appeared at first as if all the coats had been engaged in forming the aneurismal sac, but he found that the internal coat was slightly more ruptured than was consistent with its having been ruptured only at the time of the bursting of the sac. It appeared to have the character of a mixed aneurism. One of the best papers on this subject that had yet appeared had been brought before this Society by our secretary, Professor R. W. Smith, in the session 1869-70, and is published in the fourth volume of our Proceedings (New Series).—*January 11, 1873.*

Fracture of the Neck of the Femur.—DR. BIGGER presented to the notice of the Society the recent specimen of a fracture of the neck of the femur. He said the subject was a trite one, but that there was a peculiarity in this particular case which he had not hitherto observed in any instances which fell under his observation.

The individual from whom the joint had been removed was a person formerly in good circumstances, but who, in consequence of misfortunes, had been received into one of the valuable charitable institutions of this city. He had been subject to gout periodically, and had been seven months in the institution at the time of his death, which was caused by phthisis and extensive disorganization of his lungs. Eight months previous to his admission the accident occurred to his hip, making fifteen months to the time of his death. It occurred thus:—He was passing out of his house when some boisterous children at play ran against him and tripped him, so that he fell violently and unexpectedly on the edge of the flags, striking his hip on the edge of the curb-stone. He scrambled up, limped a few paces, and fell again. Carried into the house he was placed in bed, and from what he (Dr. B.) could ascertain, the nature of the accident was not discovered at the time, or if it were, no steps were taken then to treat it as a fracture. The patient himself stated that it was considered to be a severe contusion of the joint, and that rest was the best treatment. He was kept lying for more than a month, at the expiration of which time passive motion was resorted to, and some time later he was got upon crutches, and encouraged to use the limb. It gave him great pain, and he never could support his weight on the limb.

The rest of his history is very vague until his admission to the institution; even then he made no complaints about the hip, except to ask one of the physicians who was attending him for disease in the chest to give him a liniment for his hip, which he supposed to be affected with incurable morbus coxæ. Dr. Bigger, being surgeon to the institution, was then called to him. As he lay on the bed the right leg exhibited shortening of one inch; there was considerable fulness in the inguinal region, and the external aspect was much flattened over the great trochanter, and the measurement to the mesial line was shorter than on the sound side; the foot was slightly everted; motion was obtainable, but not to the full degree, in every direction, except abduction, the slightest attempt at which gave him intolerable pain; the buttock was flabby and flaccid. Stethoscopic examination gave very little evidence; except a jerking sound when the limb was stretched, there was not the least crepitus. The diagnosis was fracture of the neck of the femur within the capsule. He was very ill from the chest affection, and principally confined to bed, but he occasionally got on his crutches, and moved about, but never put any weight on the leg. He died two days ago, and the autopsy gave the following appearances:—On cutting down, the capsule of the joint was found perfect, and on opening it a considerable quantity of clear synovia flowed out; the entire neck of the bone was gone, and the shaft was closely applied to the cotyloid ligament of the acetabulum, which ligament was thickened. A joint covered with imperfect cartilage was formed between the shaft and the head of the bone, so that at first sight it seemed as if the cavity of the acetabulum was filled up and flattened; but not so, the head of the bone and the cavity containing it, with the cartilages and the ligamentum teres, were perfectly sound and normal, and there was also a sufficiency of synovia. Just below the trochanter minor, and connected with the tendons of the psoas and iliacus muscles, there was a strong prominence of bone, perhaps an exostosis, fully an inch long, which formed the peculiarity of the case, as it must have given great additional leverage to these muscles. The shortening was not due to any dislocation, but to the complete absorption of the neck of the femur. During life there had always been great pain.—*January 18, 1873.*

Cancerous Tumour in the Abdomen; Perforation of the Ileum.—DR. HAYDEN said the case which he was about to bring under the notice of the Society was one of some interest. On the 26th November a man of very small stature was sent over from Wales by a medical friend, to be admitted under his care to the Mater Misericordiæ Hospital. He was much emaciated, his age was forty-three, and the only history he could obtain was that he had been complaining of pain in the abdomen, with irregularity of the bowels for six months previously. On examining the abdomen he detected in the right iliac fossa, a hard and rugged

tumour, freely movable, and composed manifestly of a number of distinct solid bodies. It yielded the characteristic percussion note of a solid body in front of inflated intestine. The bowels were remarkably irregular, sometimes confined and sometimes rather relaxed, and the pain in the abdomen, rather remittent than intermittent, was always aggravated by a constipated state of the bowels. The alvine discharges were remarkable, presenting the appearance of liquid mortar, with a total absence of faecal odour. The pulse was very small and quick, but quite regular. The action of the heart was exceedingly weak, no impulse to be felt. The diagnosis made was, scirrhus of the mesentery, engaging that portion of the folds in the neighbourhood of the junction of the ileon with the caecum. The condition of the man gradually went on disimproving, and three or four days before his death, which occurred on the 31st of December, the pain in the abdomen became exceedingly severe and continuous, the belly became inflated, and it was no longer possible to feel the tumour. It was manifest that peritonitis had set in.

On examination of the body it was found that perforation of the small intestine had taken place near the termination of the ileon. The bowels seem to have sloughed in this situation, and the scirrhus tumour, which was large and irregular, had become quite friable; it was broken up into several masses in the process of removal from the body, and readily detached from the bowels. The portion he held in his hand gave him at first some concern, as it presented at first sight very much the appearance of a mass of faecal matter, being tinted with the colouring matter of the faeces, and led him for a moment to apprehend an error of diagnosis. He thought, in fact, that it was a case of constipation, with sloughing of the large intestine, and effusion of the hardened faecal matter into the peritoneum. The original diagnosis was, however, quite correct. This tumour was an example of scirrhus of the most characteristic kind. There was, of course, peritonitis. He should observe that the cystic duct had a small scirrhus tumour pressing on and completely blocking it up. The gall bladder was full of bile, and that was the only explanation he could offer of the want of colouring matter in the contents of the intestines. The heart was in a state of atrophy, a condition which was met with in almost every case of chronic cancerous disease, attended with general wasting. The pericardium was firmly attached to the heart, and required careful dissection to separate it; it was likewise corrugated, showing that there had been pericarditis previous to the reduction in volume of the heart. In the false membrane attaching the pericardium to the heart, a number of ossific plates were to be seen, all situated towards the base of the heart. There was also a lamina of a distinctly cartilaginous appearance, and quite elastic. He examined this carefully, and with some interest, under the microscope, for the error had been in existence for a long time, and, indeed, still

prevailed in some degree, that these bodies were composed of veritable cartilage. They were not. A careful examination showed that this plate was composed of nucleated fibrous tissue, presenting the appearance of non-striated muscular fibre under the microscope, after treatment with acetic acid. In one part there was a peculiar arrangement of calcareous particles, having the appearance of a *chevaux de frise*.

The interest of the case consisted in the following points:—Firstly, the disappearance of the abdominal tumour from the surface concurrently with the inflation of the bowels and the supervention of abdominal tenderness; in other words, of peritonitis. Secondly, the existence of atrophy of the heart in connexion with the cancerous cachexia. And thirdly, the decrease in volume of the heart subsequently to the occurrence of adhesion of the pericardium, as shown by the corrugation of the latter. It was likewise evident that atrophy of the heart was coeval with the development of carcinoma, the commencement of which the history of his illness, given by the patient, enabled us to fix at six months prior to admittance into hospital. And finally, the existence of osteoid plates, and of pseudo-cartilage in process of calcification in the false membrane uniting the pericardium to the heart.—*January 18, 1873.*

- *Putrefactive Gangrene of the Lungs.*—DR. LAW exhibited a specimen of gangrene of the lung, which he observed, true to its character, stank most abominably. There was extreme fœtor both from the breath and the expectoration. The subject of the disease was a coal porter, fifty years of age. He said that he had enjoyed good health till six weeks previously to his admission to the hospital. He caught cold, and while labouring under the effects of it he attended a wake, where he got drunk; he walked home in the rain, and remained in his wet clothes all night. His cough became very severe, and continued so; each day his condition becoming worse, until his health failed completely, and at the end of six weeks he sought admission into the hospital. When he came in, his breathing was very much oppressed, and the result of a stethoscopic examination was that there was dulness on percussion anteriorly and posteriorly all through the left lung, and posteriorly in the right lung; there was a muco-crepitating râle all through. At once Dr. Law diagnosed gangrene of the lung. The man was evidently running down, and almost moribund when admitted into hospital, and only survived three days.

The examination of the lung proved—what indeed there was no doubt of—the existence of gangrene. The left lung was in a broken down disorganized condition, what the French called *en putrilage*. Dr. Law conceived this pathological condition could hardly be called gangrene of the lung, for a bad smell was not essential to gangrene—it was more properly a putrefactive disorganization of the lung. He had seen a great many cases of this kind, and almost all come on under the same

circumstances. They came on when the individual caught cold when in a state of intoxication or at least having drank freely. The top of the lung in this case was in an especially broken down condition. There was an intensely red appearance of the trachea, and all the mucous membrane exhibited the same congested appearance. The right lung was less affected, but was still in a condition of *engouement* all through. In four cases in which this particular condition came under his observation, there was previous hæmoptysis. It was an unhealthy condition of the organ, in which there were not those precautions against hemorrhage that usually occurred in inflammation of the lung. There was one case of this kind which had come under Dr. Law's notice, and in which he took a considerable interest. He was suddenly summoned to see a gentleman thought to be in a sinking state from hæmorrhage, which was not, however, of an abundant character. He had to drive five miles to see the patient, and at once, when he entered the room, he recognized a case of gangrene of the lung. The gentleman had been cricketing, had drank largely of champagne, and lay down on the damp grass. He was attacked with all the symptoms of pleuro-pneumonia. For this he was treated actively and appeared to get better, but suddenly things took a turn, and he got worse, and on that occasion he had hæmoptysis when he (Dr. Law) was sent for. The friends of the patient, in reply to Dr. Law's inquiry, if they remarked the smell from his breath, said it was so terrible that they could not remain long in the room with him. When the regular medical attendant of the patient arrived, Dr. Law expressed his conviction that the case was one of gangrene of the lung. That gentleman said he did not see anything peculiar, but on asking him to smell the expectoration, he at once recognized the nature of the case. Hitherto the case had been treated as one of acute inflammation. He (Dr. Law) suggested that tonic treatment should now be tried, and very shortly a great improvement took place in the symptoms, and the gentleman ultimately completely recovered. This was an important point—that they should endeavour to trace out the disease, so as to accommodate their treatment to the stage of the disease. In the case which he now brought before the Society there was a profuse, green, purulent expectoration, of a most foetid character. He drank very hard, and as a result of his intemperance he had a hard nutmeg liver. He could point to many living persons, who had suffered from this disease, and who, because the disease was carefully, and accurately watched, and the exact period seen where tonic treatment should be adopted, had completely recovered. Dr. Law exhibited several preparations of gangrene of the lung. In one of these cases the blood had become extravasated into the substance of the lung, and had undergone putrefaction—one of the few cases in which putrefaction takes place in the blood, because it is exposed here to air and moisture—the conditions were essential to putrefaction.—*January 18, 1873.*

Cardiac Thrombosis : Infarction of the Lungs and Kidneys.—DR. YEO said the specimens he exhibited were taken from the body of a female, aged fifty-two, who was admitted into the Whitworth Hospital, suffering from great dyspnoea and debility. The heart sounds were weak, but no abnormal bruit could be detected. Her urine contained a considerable amount of albumen. The dyspnoea was greatly relieved, and her strength rapidly improved, under the use of tonics, counter-irritants, and nutritious diet. She left hospital on the 24th December, to spend Christmas with her friends. She returned in a week, partially under the influence of alcohol. On her re-admission she had oedema of the lower extremities in addition to her former symptoms, which were more severe than ever. The heart's action was rapid and very weak; sibilant and muco-crepitating râles were audible over the greater part of her chest, posteriorly. Four days after her admission she spat a considerable quantity of blood. The inferior portion of the right lung became perfectly dull, and the heart's action very rapid and fluttering. The dulness gradually increased; the heart became weaker; and she died on the 14th of January.

The *post-mortem* examination showed the pericardium to be healthy. The heart was large and flabby, and microscopic examination showed its muscular tissue to be in an advanced stage of fatty degeneration. The right auricle was distended with a large black semi-coagulated clot, which occupied the entire of the atrium, extending into the cavæ, and was adherent to the free surface of a hard, dry, greyish-brown mass of crumbling and laminated fibrinous material, which still tightly stuffs the auricular appendix. The dilated right ventricle contained a large, soft, moist, elastic clot, which extended into the pulmonary artery. At the apex, entangled among the columnæ carneæ, and firmly adherent to the wall of the ventricle, are some irregular masses of friable fibrin of a dull, greyish colour. The atrium of the left auricle also contained a large recent clot, attached to a fibrinous thrombus similar to that at the right side. This can now be seen still tightly filling the auricular appendix. The left ventricle is dilated and its wall thickened, but soft, pale, and flabby. It contains a tough elastic coagulation, which well shows the effect of position on the ordinary clots, which form in the cavities of the heart after death, namely, the formation of the "buffy coat" on the superior surface, while the more depending portion is almost black. At the apex can be seen an extensive, inelastic, adherent greyish mass, which is loosely connected with the large clot, but is very different in appearance and structure—resembling rather that seen at the apex of the right ventricle.

The left pleura was healthy. The right contained about three pints of thin muddy fluid, which firmly compressed the inferior lobe of the lung against the vertebral column. The pleura covering this lobe is covered by

a soft, pale yellow, fibrinous exudation. The left lung is congested posteriorly and emphysematous in front, and presents at its anterior and inferior margin two pyramids, about one and two inches high respectively, of hard congested structure which projected over the surface of the lung. The upper and middle lobes of the right lung are œdematous. The inferior lobe, which was compressed by the fluid, is perfectly airless throughout, and, on section, it is found to consist of two portions which are quite distinct in appearance and structure, and are well defined one from the other. The lower two-thirds, including the entire of the base, is dense, friable, rigid, and of a dark reddish-black colour, while the remaining portion is limp, elastic, tough, and of a dull pinkish-grey colour; from this part some clear serous fluid exudes on pressure, while from the lower part a dirty grumous, thick liquid can be pressed. The lower and larger branch of the inferior division of the right pulmonary artery is tightly plugged with a fibrinous clot throughout its entire extent. This vessel is found to supply the engorged portion of the lobe. The arteries leading to the two smaller hemorrhagic cones in the other lung are also plugged by dense adherent clots. The bronchial mucous membrane is congested throughout. Both kidneys are irregularly nodulated on the surface, and marked with deep stellate depressions. The cortical substance of each is pale and soft, and that of the left thickly studded with innumerable small greyish conical infarctions, the apices of the cones being placed at the edge of the pyramids, and their bases corresponding to the surface of the organ. The other viscera appeared healthy.

Dr. Yeo said the case was interesting as an example of the formation of what was formerly called true cardiac polypi, in contradistinction to the false polypi, or *post-mortem* coagulations which are so commonly found in the heart. These *ante-mortem* clots had been formed in this case, in all the cavities of the heart, and showed well the situation in which they usually occur in each, namely, in the appendices of the auricles, and at the apices of the ventricles. Rokitsansky and Förster think they occur more commonly in the left side of the heart, while Virchow and Lebert have found them more frequently in the right. My experience leads me to agree with the latter observers, while it forces me to dissent from the generally accepted idea that they are more frequent in the ventricles. I have found them more common in the auricular appendix of the right side than elsewhere. In many cases it is necessary to evert the appendix completely in order to find the little clot which adheres to its wall, therefore they may frequently have escaped notice in this situation, while they were noted in the apex of the ventricle. The characters which show that they were formed before death are here well marked. They differ strikingly from the ordinary *post-mortem* clots, with which every one is familiar, in the following particulars:—1st. In colour they are a dull pinkish-grey, or dirty brownish-yellow. 2nd. They are

rough and irregular on the surface. 3rd. They are localized to one part of the cavity, where they adhere firmly to the wall. 4th. They are hard, dry, and friable. 5th. They are usually more or less distinctly laminated, showing their formation to have taken place by successive depositions. These characters, which vary according to the age of the thrombus, are shared by the clots found in vessels which have been ligatured some days before death. If other evidence of their formation during life were wanted, it may be found in the remote results which they produce, and which form their chief danger. Of this we have a beautiful example in these lungs, for these hæmorrhagic infarctions, or pulmonary apoplexies, as they used to be called, are the result of the impaction of emboli broken off from the thrombus in the right side of the heart, and carried into the pulmonary circulation. The large infarction in the right lung probably dates from the hæmoptysis which occurred about ten days before the woman's death; and I think it probable that it gradually increased in size by reason of the continued deposition of fibrinous coagula on the cardiac side of the original embolus, and thus branch after branch leading from the plugged vessel became choked and filled by coagulum. For Virchow has proved that when an embolus becomes impacted, it causes coagulation to go on immediately around it, and thus it grows by additions to its distal and proximal extremities. The vessels beyond the embolus at the same time become gradually filled with the blood attracted into them through the collateral circulation, and the *vis a tergo* being removed, they are unable to propel this blood onwards towards the heart, and thus the veins in their turn become injected and the tissue is engorged as we see it here.

The infarctions of the left kidney are probably of older date, and I think their number is best explained by the fracture of one friable embolus into numerous pieces which are forced along by the powerful renal circulation, and impacted in the vessels where they enter the cortical portion of the gland.

There was no valvular disease, or roughness, or irregularity of the walls of the cavities to account for these extensive clots. The great weakness of the heart's action, and the localization of the clots to those positions where the motion of the blood is least rapid, show that the coagulation probably depended upon retardation of the circulation arising from fatty heart. The albuminuria and the old disease of the parenchyma of the kidneys, suggest that the chemical condition of the blood itself may have had some influence in assisting in the formation of the coagula—as Paget has suggested—(*Lancet*, July, 1845).—*January 18, 1873.*

TRANSACTIONS OF THE CORK MEDICO-CHIRURGICAL SOCIETY.

President—DR. FINN.

Hon. Secretary—DR. HOLMES.

Remarks on the New Views regarding Pulmonary Phthisis. By W. JACKSON CUMMINS, M.D., Physician to the Cork South Infirmary and County Hospital, &c., &c. Read January 8th, 1873.

MR. PRESIDENT—The accumulated experience of every physician in this country overflows with facts bearing on the etiology, pathology, and therapeutics of pulmonary consumption; the disease which of all others most arrests our attention, and draws forth our sympathies. It is, therefore, before societies of clinical physicians, on whom devolves the heavy responsibility of applying practically the opinions and theories of the day, that all new views ought to be pleaded, and by the verdict of which they must stand or fall. It is only histologists, minute anatomists, and experimentalists, who can decide disputed points between the eminent men who devote their time and talents to those difficult studies which have shed such lustre on our profession, and thrown such a clear light upon the medical science of our day; but, as the results of such investigations, if worth anything practically, must be confirmed by clinical observation, every practitioner can draw his own inference regarding "new views," and is more likely to be unbiassed in the opinions he forms, than those accomplished men who view clinical facts through their own experimental investigations. Hence the importance of not letting another session pass without an appeal to this Society for its opinion regarding the great medical question of the day. You are all doubtless aware that the old doctrines of Broussais and his followers, and the opinion held generally by the profession and the public, more than a generation ago—that phthisis proceeds from a "neglected cold"—was completely superseded by the teaching of Laennec and his disciples, that every form of the disease is preceded by a specific neoplasm of which the lung affection is only a local manifestation; and that this theory was almost universally accepted until Virchow and his followers, within the last few years revived the old doctrine in a new form, and "translated," as Dr. Walsh expresses it, "the doctrines of Broussais and Addison into microscopical language." I suppose there are few clinical physicians who have not observed numerous cases of phthisis which suggested doubts as to the universal applicability of the narrow views of Laennec, and yet so implicit was the faith in the dicta of that

admittedly great pathologist, that one* only had the courage to raise a dissentient voice. The writings of the modern school have fallen like a thunderbolt on this blind faith in the great discoverer of the stethoscope: and few, if any, works on medicine of any day, have arrested more attention, or obtained more rapid circulation, than Niemeyer's classic work on consumption. Such a desire for new views is some evidence that grave doubts existed regarding the old; and when it may be added that in spite of some adverse criticism, the doctrines enunciated by Niemeyer have effected almost as great a revolution in medical opinion regarding phthisis, as the works of Todd and his confrères did in the treatment of acute diseases, a few years ago, it must be admitted that they have some practical value. The tide of medical opinion has already set in favour of the "new views," and even now there is danger of too great a reaction against the old: we want reform, and not revolution; we have seen too much of the latter since the days of Todd. I wish it had fallen to the lot of some one with more time and ability at his disposal to bring this question before you; but it is better that even an imperfect essay such as this should be discussed, than none at all.

It is not denied by modern pathologists that a small proportion of phthisical patients are primarily tubercular, but such cases are regarded as so small a minority, as not to be taken into account in dealing with the question of phthisical pathology; and, moreover, it seems to me that the few cases, admittedly tubercular, are not described as presenting the symptoms and physical signs of ordinary phthisis, at least until after they have undergone a metamorphosis into the caseous degeneration which causes pulmonary ulceration—a metamorphosis which they may or may not undergo, and without which they may destroy life. The granular miliary tubercles, which are the distinguishing feature of this class of cases, may be the offspring or parent of the cheesy degeneration which precedes phthisical excavation; when the former, it is said that they are the result of blood charges due to an internal inoculation from the caseous masses, or from ulcerous cavities; and when the latter they, themselves undergo the cheesy degeneration in the same way that it is said hæmorrhagic infarctions, inflammatory exudations, and even cancerous nodules become transformed, before they cause destruction of the lung substance. But it is said that the vast majority of phthisical cases met with are not primarily tubercular, that the worst thing which can happen to them is that they should become so, and that numbers of person die of phthisis without ever having had a tubercle in their lungs or in any other organ. The pulmonary phthisis or excavation in these cases is described as the result of various local diseases and inflammations,^b diverse in their etiology and pathology, but having a tendency *common to them all*, though in varying degrees, to caseous degeneration.

* Dr. Addison.

^b Niemeyer, p. 207.

Having arrived at this cheesy condition, the heterogeneous products become homogeneous, and losing all the characters which had previously distinguished them, undergo further transformation of a retrograde or progressive character; when the former, "the caseous masses become still more inspissated, and the shrunken atrophied cells break down into a detritus,"* "little by little their organic matter disappears, while calcareous salts are deposited, until there finally is left a chalky or mortar-like concretion." In other cases the arrested fatty metamorphosis of the cells is re-established, they become liquified and capable of re-absorption. The absorption takes place "through supplementary fatty degeneration and liquefaction, and may be so complete, that upon dissection nothing is found but pulmonary tissue in a state of induration from interstitial pneumonia."

But when caseous degeneration undergoes progressive transformation, liquefaction takes place speedily, and, "together with the tissue, breaks down into a creamy puruloid matter." This is possibly brought about through the crowding together of the accumulated cells in the air vesicles, partly shed there, and partly inhaled from the bronchi, "encroaching on one another," exerting a pressure upon surrounding tissues and their vessels, "and thus producing anemia and necrosis of the pulmonary tissue" (pp. 217, 218). But although it is held that a great variety of local lung diseases may thus produce caseous degeneration and phthisis, by far the greater number of cases are due to chronic catarrhal pneumonia, and chronic bronchial catarrh; where, "owing to the slowness and tedious nature of the progress, the tendency of which is to a perpetual accumulation of young cells in the air vesicles, the cells are more and more crowded together and become mutually injurious and more liable to degeneration." Another process by which a great variety of local lung affections, including all forms of pneumonia, collapse of lung, tubercle, cancer, hæmorrhagic infarctions, abscess, chronic bronchitis, etc., may give rise to phthisis, is by their causing or becoming associated with chronic interstitial pneumonia which is described as "a hyperplasia of connective tissue, resulting in augmentation of the substance of the lung, and diminution of its cavities for the reception of air." At first, "soft and filled with blood," the tissue afterwards contracts and is transformed into a "callous, bloodless substance," which draws in the thoracic wall and dilates the bronchi. When these latter become sacculated and are situated in the lower lobe of the lung, "not being compressed even in coughing," their secretion accumulates and decomposes, corrodes their walls, producing "slough, inflammations, or diffused putrescence of lung and hæmorrhage."

This form of inflammation rarely exists as a primary disease, but it

* The quotation marks here, and elsewhere, except when the author's name is mentioned in the margins, refer to "Niemeyer's Text-book of Practical Medicine."

accompanies most cases of consumption, and causes many of its physical signs. The experiments of Villiman, Lebert, Cohnheim, Fränket, and Sanderson have established the fact that tubercle may be inoculated, and the further important point that inoculation with the products of ordinary inflammation and other organic matters, or even with inorganic matters capable of exciting inflammation, may infect the blood so as to give rise to a general outpouring of grey granulations in the lungs and other organs. It seems, however, to be pretty generally admitted now that there is no such thing as a tubercle cell, nor any specific histological element characteristic of a tubercular diathesis. Virchow (quoted from Walsh) says, "that the cells present in tubercle may be relics of epithelial cells, or of some other natural product, or they may be related to or identical with exudation cells, or pus globules, or to and with the cells occurring in cancerous growths or in typhoid infiltrations." While Sanderson says that "there is no difference structurally between the induration of phthisis and of chronic pneumonia." "No anatomical elements in the hard parts of a phthisical lung which are not found in the hard parts of chronically irritated tissues in any other organ, and no elements in the induration of phthisis which are not to be found in chronic bronchitis." Nay, more, it is even possible that there are no anatomical elements in the induration of phthisis which are not to be found in healthy lung. This author considers tubercle in serous membranes a hypertrophy of gland-like or adenoid structures, and in the lungs a new substance identical with it, produced in the inter-alveolar spaces, giving rise to a sort of lobular pneumonia, which becomes cheesy. We cannot wonder that pathologists who have obtained from minute investigation such a mass of evidence in favour of the non-specific nature of phthisis should deny it a place among the diathetic diseases, and endeavour to supply a new explanation of the statistical evidence which places the disease high on the list of hereditary transmissible affections. Those pathologists who argue that phthisis is primarily a local disease, admit a predisposition hereditary or acquired, but deny to such predisposition any specific character. They say that all causes which "lower the nutrition and vital resisting power of the system," which "retard or disturb the normal development and conservation of the organism," which produce "feeble power of resisting noxious agents," a "weakness and vulnerability of constitution," are predisponents of those low forms of inflammation which result in the proliferation and crowding together of "young, indeterminate, decrepit, and perishable cells, incapable of making healthy tissue," or of repairing perfectly even the more sthenic kinds of inflammation, and that products of local disease, not removed in consequence of this imperfect repairing power of the tissues and organs, undergo caseous degeneration, and result in phthisis.

These ingenious theories are undoubtedly true in many instances,

and it may well be added that a feeble power of resisting disease is not incompatible with a high degree of physical development; rather, indeed, the contrary, as there is sometimes a greater disproportion between the organs of nutrition and the amount of tissue to be nourished in the robust than in the delicate; but while we are ready to admit the theory, and even accept the explanation, we cannot go so far as to believe that they disprove a SPECIFIC blood tendency or diathesis, antecedent to the local changes which cause pulmonary phthisis. Even assuming that a tendency to the proliferation of cells and to caseous degeneration is the predisposition, it is surely as deserving the name of a diathesis as is the tendency to formation of lithic acid in gout. But although I believe that as far as predisposing causes are concerned, the ground is merely shifted between the supporters and opponents of the "new views," it is not so when we come to consider the local disease itself, which one party maintains to be a non-inflammatory deposit from the blood, and the other a transformed exudation, and here, in my opinion, clinical experience tends altogether in favour of the "new views," for, if we take as an illustration six brothers who have inherited what is called a tubercular diathesis, and carried it to adult age in a latent form, with nothing apparent to distinguish them from their healthiest neighbours; three of them may carry their immunity from phthisis to a good old age, and the other three may catch cold, suffer from chronic catarrhal pneumonia or bronchitis, become phthisical, and die. I believe that the vast majority of phthisical patients can furnish some such history of catarrh or other local cause capable of lighting up the predisposition or diathesis, by whichever name you call it, which they have inherited or acquired, and that apart from such local exciting cause the predisposition may be as harmless as a mine of gunpowder without the spark of fire. I cannot help thinking besides that there is a something more specific in the majority of cases of phthisis than a mere tendency to catarrhal inflammation of a low type, a something hitherto undescribed, which has an affinity for the apices of the lungs; a something which renders inoculation with phthisical matter far more likely to produce tuberculosis than with any other organic or inorganic substance; a something which is infectious, and may be communicated by close and continuous contact from the sick to the healthy, and which makes us dread the most trifling depressing influence for some of the families we attend, and fearless about others even when more exposed, and I am sure we could all furnish family histories, with intermarriages, etc., to justify the sort of instinct with which we cling to the old theory of a diathesis.

There is no part of Niemeyer's book which seems to me more borne out by clinical observation than his views regarding interstitial pneumonia, to which I have already alluded. They explain many of the symptoms

and physical signs met with in bronchitis and old catarrhs, as well as in phthisis, and scarcely a day passes without my seeing cases which tend to confirm them. The explanation, too, which he and other modern observers suggest as to the cause of the enlargement of the superficial veins which are so frequently seen ramifying over the thorax in phthisis, and the close connexion through the intervention of organized pleural adhesions between the deep and superficial circulations, are peculiarly interesting, while the latter explains the great therapeutic value of inunction of the skin of the thorax with cod-liver oil and other nutritive agents. If it be a fact that fatty degeneration of the products of inflammation must precede their absorption and excretion, and that failing a metamorphosis of that kind, caseous degeneration, and destruction of the lung will occur, the importance of a *direct* blood communication between the skin and the lungs cannot be overlooked, nor the advantage of supplying those organs with fats and oils through such ready channels. I have endeavoured in this brief essay to condense as far as possible a few of the principal "new views" enunciated by modern authors, and especially by Niemeyer, whose clear and lucid explanations have formed the base of the points I have touched on. I have not followed him and others into the detailed descriptions of the mode in which the inhalation of irritating particles of iron, coal, dust, &c., &c., produce phthisis, as these matters cause excavation of the lung, through the intervention of inflammation, followed or not by caseous degeneration, subjects which have been before us in a general way, and which I have not space to enter into more fully, but there is one more subject which cannot be passed over without at least a brief consideration, viz., pulmonary hæmorrhage. It is the arteries, veins, and capillaries of the pulmonary artery which make the lungs the most vascular organs in the body, and it is from one or other of these sources that we would expect bleeding to take place in phthisical hæmoptyses. Such, however, is not the case, for the blood of the pulmonary artery is the blackest in the body, and expectorated blood is of a bright arterial tint, except in those rare cases where aneurisms of the pulmonary artery, or want of support of some large artery lying on the wall of a cavity, cause rupture and hæmorrhage of black blood. But in the large proportion of cases it is held by modern pathologists that the branches of the pulmonary artery and the capillaries and veins are gradually obliterated as phthisical disease progresses, and that it is from the bronchial (nutritive) arteries that even profuse hæmoptysis occurs, in the same way that blood pours from the mucous membrane of the nose in epistaxis. In some cases bronchial hæmorrhage is caused by weakness of the walls of the blood vessels, and if slight in amount and fully expectorated may do no harm, or if hyperæmia be present, may even do good, and be a relief; but in many cases a portion of the blood is drawn

during inspiration into the alveoli, and may lodge there, undergoing caseous degeneration, with softening and excavation, giving rise to surrounding inflammation and irritative fever. Clinical experience certainly tends to confirm the old opinion set aside by Laennec's teaching, and thus recently revived, of "phthisis ab hæmoptoë" in a certain number of instances, but I believe that in the vast majority of cases catarrhal inflammation precedes hæmoptysis, as it so frequently does epistaxis, and that it is due rather to the intensity of the inflammation of the mucous membrane, or to a poverty of the blood itself, than to weakness of the vascular walls; however occasioned it may give rise to inflammation if retained, which clinical observation leads us to believe it rarely is, unless the hæmorrhage is so profuse as to cause weakness or faintness and temporary paralysis of the power to expectorate. I have seen hundreds of instances of slight phthisical hæmoptysis in patients who walk daily to the hospital, and are nothing the worse for it. In the immense majority of cases my experience leads me to believe that bronchial hæmorrhage does no harm unless it continues for days in a profuse form, and then it lights up one of the most dangerous forms of inflammation with high irritative fever, followed by diffuse inflammation or granular infiltration, followed sometimes by rapid excavation of the lung and all the symptoms of acute phthisis. Patients who have suffered from this form of hæmorrhage rarely live long, and in all cases the prognosis is bad. I may just mention incidentally that I can only remember two or three cases in my own practice where phthisical hæmoptysis caused sudden death by obstruction of respiration and asphyxia.

I will now conclude, Mr. President, by stating my opinion that the "new views" regarding pulmonary phthisis are a great boon to practical physicians. They have imparted a special interest and study to every individual case of the disease, and withdrawn a large number from the hopeless group of the doomed to die, besides giving us two grand principles to guide our treatment, viz.:—1st, to prevent and subdue local inflammations, and 2nd, to build up the nutrition of the system, to increase its power of resistance, and enable it to form healthy cells and good sound tissues.

Hypertrophy of Fetal Kidneys, causing difficult Labour in Breech Presentation.

By W. JACKSON CUMMINS, M.D., Physician to the Cork Maternity, &c. Read before the Society Jan. 28th, 1878.

MR. PRESIDENT,—The specimen which I lay before the Society was taken from a foetus immediately after birth, and is interesting, not only as a pathological curiosity, but because such excessive overgrowth of the

kidneys, presenting a formidable obstacle to the expulsion of a breech presentation, is, to say the least, unusual.

The case, as reported by Mr Gun, the student in charge, is as follows:—

"Mrs. —, a maternity patient, who had previously suffered from two difficult labours, was taken with her third labour on the evening of the 22nd of last December; and as little progress had been made on the afternoon of the following day, Dr. Cummins, the physician of the week, was sent for and diagnosed a breech presentation entering the brim of the pelvis in the right oblique position, the anus of a female child being felt posteriorly, and the vulvæ anteriorly. The os uteri was almost quite dilated, and the pains recurred at intervals of about five minutes, short but severe. As the patient was strong, and patience alone seemed necessary, Mr. Gun, the student on duty, was left in charge."

"Dr. Cummins returned at 7.30 o'clock with Dr. Leech, the resident maternity physician, and finding that little or no progress had been made, gave one or two doses of ergot, which increased the pain, without advancing the presentation to any great extent; he therefore passed a blunt hook over the thigh of the fœtus, and made traction during the pains. Very little progress was made, as the fear of fracturing the femur forbade the use of sufficient force to overcome the obstacle which it was now plain existed. Dr. C. then got the first two fingers of his left hand over the same thigh, and removed the hook. Powerful traction was now made, and as the presenting part came more within reach he passed the two first fingers of his other hand round the other side of the thigh, and increased his efforts. As soon as the breech had passed the vulva the cause of the delay became apparent, as the abdomen was found to be enormously increased in size, and so firm as to be incompressible. It was extracted with great difficulty. The face then turned into the hollow of the sacrum, and the head was easily delivered. The child was dead."

"There was delay in the third stage, and considerable hæmorrhage, so that Dr. C. had to remove an adherent placenta from the uterus. The mother recovered perfectly."

On opening the abdomen of the fœtus I found these kidneys, which are about the size of ordinary adult kidneys, and which were much larger before they were macerated in osmic acid and caustic potash. The diminution in size has probably been caused by the absorption of the infiltrated fluid. It is not easy now to see the structure of the kidneys, which seems to have been a simple overgrowth of the renal tissue, with thickening of the capsule, and some flattening of the surface.

I have examined all the authors I could find and can find no parallel to this case, and my friend Mr. Leech, who has kindly shown me the specimen, and who is a member of the College of Physicians, has kindly shown me the same specimen, and has kindly shown me the same specimen, as he has shown me the same specimen.

shall read you a few extracts from a note I have just received from him. He says:—"I have looked up a number of continental authorities, and no notice has been made by any observers that I am aware of, of the dymetrical excess of formation exemplified in your specimen. Rokitansky says: 'Excess of development occurs *very rarely*, except in the case of biventral monsters, where it is generally a *third kidney*.'" I regret much that these kidneys were not weighed at the time of delivery, but the students were out of town in consequence of the Christmas holidays, and I had not time to see about the specimen myself; but even this imperfect sketch is not devoid of interest.

Case of Retroflexion of the Uterus, with Remarks. By W. JACKSON CUMMINS, M.D., Physician to the "Cork South Infirmary," and the "Cork Maternity," &c.

SOME time since I was consulted by a young married lady, who had borne children, but had suffered the last two years from retroflexion of the uterus, for which she had been under treatment, and had worn pessaries, without relief.

Her condition was a truly pitiable one, as she was always in pain, even when at rest in any position, and the "uterine lameness," as Dr. Graily Hewitt calls that peculiar affection which is produced by pressure on the nerves within the pelvis, made walking painful and difficult. Her chief suffering was caused by a dragging painful sensation at the right side of the abdomen, between the umbilicus and crest of the ilium.

On examination I found the uterus enlarged, and the fundus bent backwards and to the left side, where it was fixed, and could only be moved upwards by steady firm pressure, which caused much pain.

I scarified the os uteri freely after a plan I first saw adopted in Dr. Greenhalgh's clinic at St. Bartholomew's (and which I have since used with benefit), and prescribed the iodide and bromide of potassium. A few days after I introduced a Hodge's pessary, which at once removed the pain and lameness, so that she could walk about well and without pain. The relief, however, was transitory, as the support gave so much pain that the remedy was worse than the disease, and I had to remove it.

I subsequently scarified the os on two or three occasions, and tried a great variety of pessaries of various shapes and sizes, with spring and without, and with so little success that I almost gave up the case in despair, when one day it occurred to me to puncture the os deeply with a "needle-knife" in the manner recommended by Dr. Lombe Atthill, in his work on diseases of women.

I had several times punctured the os uteri in this manner before, but I never saw such hæmorrhage from the puncture; the blood spouted in such profusion as really to alarm me, and though I was amply

provided with cotton wool, and at once plugged the speculum, I was unable to arrest it before the patient had fainted.

When she came to, the hæmorrhage had ceased, and did not return, and she expressed herself much relieved, though weak.

A few days afterwards I introduced a Hodge, which she bore without any difficulty, and many weeks after her mother wrote to me, thanking me for the unexpected cure I had made of her child. Once since this lady called on me to ask if she might ride on horseback, and said that she was as well as ever, and able to do anything.

Her graceful carriage and bearing, the rapidity and ease of her movements, and her blooming health all contrasting so forcibly with the bowed down, suffering figure of "other days," confirmed her assertions, and made me almost doubt her identity. I asked her to allow me to remove the pessary to wash it, and try how she would get on without it for an hour or so, but she would not part with her "friend" even for a moment, and she still wears it, and still enjoys perfect health; at least I have not heard a word to the contrary.

I do not bring forward this case, Mr. President, as anything new, but simply because it bears upon a point in the pathology of uterine flexions upon which different opinions are held by eminent gynæcologists.

I believe all consider that congestion, and sometimes inflammation, of the uterus accompanies flexion of the organ, and that a large proportion of flexions arise during the puerperal month, or after abortion, from premature exertion and imperfect involution. In such cases the soft vascular condition of the organ predisposes its fundus to displacement, so that congestion not only precedes flexion, but is one of its most efficient causes. The same may be said of flexions due to straining or over-exertion during menstrual congestion. In all these cases the congestion may pass away, leaving the flexion behind which it has produced. It is generally supposed, however, that the flexion itself, by producing strangulation at the sharply bent portion, may cause congestion—a congestion capable of being relieved by simply restoring the fundus to its natural position, and keeping it there, so that the strangulation is relieved. In a large proportion of the cases of retroflected uterus which I have seen there has been great congestion or inflammation, and extreme tenderness of the fundus when pressed upon. Such cases do not bear a pessary well, and free depletion of the organ must be effected before any mechanical support is tolerated.

The unusual hæmorrhage which puncture with Hall's knife produced in the case I have read was due to an unusual amount of congestion, and though the suddenness of the loss caused fainting, the relief experienced was immense, and its effect upon the disease most satisfactory. I have met with cases of retroflexion which were immediately and permanently cured by a pessary without any previous depletion of the organ, but in

by far the greater proportion of cases local depletion in some form or other is necessary, and must precede mechanical support.

In a few cases depletion alone may be sufficient to effect a cure, as there seems a natural tendency in the uterus to right itself when its congestion is relieved, but as the pessary admits of the patient moving about at once, and as without it rest on a sofa is indispensable for some time, the instrument is generally preferred.





DR. MAPOTHER ON ICTHYOSIS.

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OF

MEDICAL SCIENCE.

JUNE, 1, 1873.

PART I.

ORIGINAL COMMUNICATIONS.

ART. XIII.—*A Case of Ichthyosis.* By E. D. MAPOTHER, Surgeon to St. Vincent's Hospital; Professor Royal College of Surgeons; late Examiner in Surgery, Queen's University.

DR. ADRIEN, of Oldtown, County Dublin, lately sent into my wards the following exemplary case of that very rare disease, ichthyosis:—

Thomasina F., aged forty-two, the mother of seven healthy children, now aged from twenty to two and a-half years, found in August, 1870, that her legs were swollen. An eruption, which, from her description, appears to have been eczematous, followed, and remained till her confinement in November. She then became quite healthy, and suckled her child for eight months. About June, 1871, the ichthyotic condition of the skin began to show itself, and it has increased up to the present date. The cuticle on the backs of the forearms and front of the thighs and legs is of precisely the colour of the upper surface of the turbot, and is divided into quadrilateral masses, varying in width from one and a-half lines to half a line. This cuticle can be picked off in plates of about two lines in thickness, and the underlying skin is rough, red, and sometimes apt to bleed. When the wrist is extended the skin rises into great transverse wrinkles, and by taking one of these between the finger and thumb it can be found that the true skin is greatly thickened. These conditions are equally marked about the knees and ankles. On the front of the abdomen and chest and entire dorsal surface of



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FROM THE

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Mr. Lesage, accompanying this brief
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Windsor's Hospital, where she can be



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Thomasina F., aged forty-two, the mother of seven healthy children, now aged from twenty to two and a-half years, found in August, 1870, that her legs were swollen. An eruption, which, from her description, appears to have been eczematous, followed, and remained till her confinement in November. She then became quite healthy, and suckled her child for eight months. About June, 1871, the ichthyotic condition of the skin began to show itself, and it has increased up to the present date. The cuticle on the backs of the forearms and front of the thighs and legs is of precisely the colour of the upper surface of the turbot, and is divided into quadrilateral masses, varying in width from one and a-half lines to half a line. This cuticle can be picked off in plates of about two lines in thickness, and the underlying skin is rough, red, and sometimes apt to bleed. When the wrist is extended the skin rises into great transverse wrinkles, and by taking one of these between the finger and thumb it can be found that the true skin is greatly thickened. These conditions are equally marked about the knees and ankles. On the front of the abdomen and chest and entire dorsal surface of

the body the skin presents the characters of xeroderma, a few minute red cracks being apparent at points of flexure. The hair is thinned, and the scalp and forehead are covered with thin yellowish scales of dried sebaceous matter. On the cheeks, especially the right one, there are crusts of impetiginous matter and sebaceous secretion, and the lower eyelid is slightly œdematous. These conditions the patient attributes to frequent washing and rubbing. The only places where the skin is quite healthy are the axilla, lower half of the breasts, the groin, the inner surface of the thighs, and the palms and soles. Perspiration occurs in these places, but elsewhere the skin remains perfectly dry. Other secretions are decreased, the urine averaging only twenty ounces daily, and the bowels not moving oftener than once in five days. She menstruated normally till August last, when severe menorrhagia occurred, and the function then ceased. Warm weather distressed her more than cold, contrary to what is noted of other cases. The only thing she complains of is panting and palpitation of the heart on taking exercise. The pulse is very full, 84 per minute; the first cardiac sound is very loud, and the precordial dulness is abnormally extensive. The checking of secretion and irregularity of circulation in the cutaneous vessels must lead to hypertrophy of the heart in cases of extensive ichthyosis. Dr. Church, of St. Bartholomew's Hospital, has recorded the case of an undergrown girl of fifteen, whose heart, after her sudden death, was found to weigh fifteen and a-half ounces.

Authors distinguish two essential varieties of ichthyosis—the sebaceous, which is more correctly an extreme degree of seborrhœa, and the papillary, in which the cuticle, the pigmentary layer, the papillæ, and the corium are all hypertrophied. I do not attach importance to rigid classification of skin diseases, being more interested in their etiology and treatment, but I must remark that ichthyosis has no claims to belong to the *squamæ*. It is essentially an hypertrophy, and in the case I am detailing the sebaceous glands would seem to have undergone the increase in the scalp and face, places in which they greatly abound, and elsewhere the papillæ and super-imposed cuticle have enlarged, consequent on greater afflux of blood and greater action of the nutrient nerves. The anatomical characters of papillary ichthyosis, it would appear from the investigations of Kohn, are not very different from those of what are vulgarly called "seedy warts," condylomata, epithelioma, or Marjolin's ulcer. The area of diseased action is, however, always more extensive. Scales of cuticles heap in layers over the

extremities of the papillæ, which are separated by deep vertical fissures.

The causes of the affection are as obscure as when it was first described by Paracelsus. My patient was a robust cleanly woman living at the seaside; her family was remarkably healthy, except one sister, from whom a cancerous breast was removed in St. Vincent's Hospital fifteen years ago. I may remark that she remains in perfect health. Ichthyosis is usually congenital or developed shortly after birth; my patient describes her skin as being, till she was forty, as soft and as fair as she could without vanity admit. Neither does the disease seem in her case hereditarily transmissible. As it has been stated that vaccination does not succeed in the ichthyotic, I may mention that all her seven children took the cow-pock infection very well. The disease is said to be twenty times as frequent in males as in females, and to be hereditary only in male or female lines. That maternal mental impressions, animal food, alcohol, and other pathological agenda exclusively human, produce ichthyosis, would seem to be disproved by its occurrence in a calf, from which Harpeck obtained his well known microscopic section.

The differences between the sebaceous and the papillary varieties are well shown by the results of treatment—soaking with alkaline poultices, and washing with soft soap will cleanse off the scabs of the former, but as to curing cases of the latter in which the papillæ, pigmentary layer, and cuticle, are hypertrophied, one might as well hope to scrub a black man white.

My patient has, however, obtained some relief from daily tepid baths, containing six ounces of washing soda and inunction with equal parts of glycerine and cod-liver oil. This oil (first advised in such cases by Dr. Banks), is very penetrating and emolient, and for some such reason was used by saddlers with their leather long before its introduction as a medicine.

The faithful photographs by Mr. Lesage, accompanying this brief notice, exhibit most of the features I have detailed.

The patient is still in St. Vincent's Hospital, where she can be seen by any member of the profession.

ART. XIV.—*Successful Case of Excision of the Scapula.* By JAMES SPENCE, F.R.S.E.; Surgeon to the Queen in Scotland; Professor of Surgery, University of Edinburgh.

JOHN DOW, aged sixty-eight, was first admitted to the Royal Infirmary under my care on the 25th of December, 1871, on account of a tumour in the scapular region. He stated that the swelling had existed for upwards of twenty-three years, and that about twenty years ago he had consulted Mr. Syme, who told him it might never trouble him, and as it gave him no pain he did not have it removed. He continued to work at his ordinary occupation as a saddler, without any pain or inconvenience, till about six months before his admission, when the tumour began to increase rapidly in bulk, and also became painful. At the time of his admission the growth was the size of a melon, somewhat flattened, and occupied the posterior aspect of the scapula in the infraspinous fossa. It was rather movable, but felt fixed near the neck of the scapula. He stated that with the exception of cough and occasional attacks of "rose," his general health was good.

After he had been in hospital for some days he was attacked with shiverings, severe bronchitic cough, and derangement of the digestive organs, which conditions culminated in an attack of erysipelas of the leg. I therefore advised him to leave the hospital for a time, and to return when his health was improved. He accordingly left on the 25th of January, and was again admitted on the 19th of February, 1872.

On re-admission he stated that the swelling had enlarged very much towards the armpit, and that the pain there and down the arm was so intense that for some nights past he had got no sleep. With the exception of the old cough, his general health was now better, but the sudden increase of bulk and the pain made him desirous to have the tumour removed at all hazards.

On examination I found that whilst the part of the tumour in the infraspinous fossa of the scapula was still pretty movable and not much altered in size or form, there was a very marked enlargement towards the axilla, where it pressed forwards, surrounding the neck of the scapula and the shoulder-joint, and nearly filled the axillary space. At the neck of the scapula the growth seemed fixed. I therefore gave it as my opinion that excision of the scapula would probably be required. As the patient was desirous

that the operation should be performed as soon as possible, I proceeded to operate on February 21st.

Operation.—As the tumour had originally been of slow growth, and had remained seemingly movable up to a recent period, I thought it advisable, before proceeding to excise the whole scapula, to make my incisions so as to ascertain whether it was firmly attached to the bone or merely bound down by muscles and fascia. I accordingly began by making an incision from the posterior part of the acromion process obliquely downwards and backwards over the tumour to beyond the inferior angle of the scapula, dividing freely the fascia and fibres of the infraspinatus muscle. This exposed the scapular aspect of the tumour, smooth, defined, and so movable that it seemed as if it would have been easily turned out; but at the neck of the scapula and around the glenoid cavity the growth was evidently from the bone or periosteum, and was soft and cerebriform in substance, so that I proceeded to excise the whole scapula. With this view I entered the point of my bistoury over the clavicle internal to the attachments of the coraco-clavicular ligaments, and carried the knife backwards over the acromion into my first incision, so as to complete a long incision from over the outer part of the clavicle to the inferior angle of the scapula. I next made an incision from the superior angle of the scapula obliquely downwards and forwards over the tumour towards the neck of the scapula. The flaps of skin were then dissected and reflected, and the attachments of the trapezius and deltoid muscles to the spine of the scapula divided. I then cleared the clavicle by separating the attachments of the trapezius, deltoid, and great pectoral muscles, and divided the bone, dissecting through the coraco-clavicular fascia, so as to enable the axillary artery to be completely commanded. I now hooked my left forefinger under the coracoid process, and divided the attachments of the pectoralis minor, short head of biceps and coraco-brachialis muscles. The disarticulation of the humerus was next accomplished, and this required care, as the projection of the tumour around the joint made it necessary to be very cautious. The long head of the triceps was then divided, the scapula being carried backwards and drawn from the body, so as to allow me to divide the rhomboid and other muscular attachments with a single sweep of the knife. The loss of blood was very small, for in dissecting off the cutaneous flaps, and in separating the deltoid and trapezius from the scapula, all bleeding points were secured before proceeding farther. Sub-

sequently, from the manner in which the axillary artery was commanded, no bleeding occurred, the subscapular and other vessels being secured before compression was relaxed. I expected to have had bleeding from the artery of the base of the scapula, but could scarcely find a bleeding point in its usual position.

Although there was so little loss of blood, the effect of shock was very obvious when the scapula was removed, as evinced by feeble pulse and sunken features. But this state speedily disappeared, and before the wound was dressed the pulse had recovered after the administration of stimulants.

After all bleeding had been arrested, I examined the wound carefully, and noticed a series of elongated bodies of a prismatic form and a milk-white colour in the position of the axillary chain of glands, and these I dissected out. (They proved on examination to be glands, but contained no proliferous cells.) The whole surface was then washed with tepid carbolized water, and the flaps of skin having been brought together by sutures, a long flat pad of lint enclosing "marine tow" was placed in the axilla and along the inside of the arm, which was then secured to the side by a broad flannel roller. The lines of incision were dressed with strips of dextrine oiled silk, wetted in carbolic lotion, and secured by gutta-percha tissue glued down at the edges by touching them with chloroform.

The following extracts from the report in the Hospital Journal will best show the subsequent progress of the case:—

"February 22nd.—The patient has passed a tolerably good night; pulse 120; little discharge from the wound. He is getting milk and white of egg for diet, and occasional small portions of brandy and water and ice to allay thirst.

"24th.—Patient much as at last report, but troubled with severe bronchitic cough. Two stitches were removed to-day. The wound is looking very well, and there is only very slight discharge.

"25th.—Patient has had a good night, but complains of soreness of his mouth. The tongue is coated with a white film, and has fallen to 108, and is of a milk-white colour. Diet, milk and white of egg, beef-tea, and brandy. A solution of sulphuric acid in water to be used.

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to-day, and the greater part of the incisions corresponding to the back of scapula seem united by first intention. There is a little irritation and discharge at the wound over the section of the clavicle, and the head of the humerus has a tendency to press against the cicatrix at the upper part, causing it to break up.

" March 1st.—Patient going on favourably. Cough less. The tongue is slightly furred; pulse 108, regular, and good strength. Ordered a cathartic, as his bowels have been confined.

" 4th.—Patient complains of feeling weak and is very irritable, but his general condition is good. Pulse 104; temperature of skin natural. Diet, steak or chop, with sherry 4 oz., and 2 oz. of brandy.

" 11th.—Patient is more cheerful; is still troubled with his cough, but takes his food well. Pulse 100. All stitches have now been removed some days since; all the large wound has closed, with the exception of the upper part, where the head of the humerus still tends to project, no matter how the arm is placed, and even when he is sitting up in bed with the elbow unsupported.

" 13th.—Pulse 90; little or no discharge.

" 23rd.—Progressing favourably.

" 26th.—Patient has had a fresh attack of bronchitis, which has rather exhausted and dispirited him. The wound is healed, except at upper part, which is now contracting.

" April 13th.—Patient allowed to get up. Wound nearly healed over head of humerus. His cough is greatly better.

" May 7th.—Dismissed cured."

Remarks.—The cases in which excision of the entire scapula has been successfully performed are as yet few, and such instances as that just recorded are worthy of publication as encouragement to operate. The foregoing case has, moreover, some interest in other respects as regards the history and character of the tumour, and as drawing attention to the operative procedure which should be adopted to facilitate the after treatment of the wound.

The tumour for which excision was performed presented some peculiarities when we contrast its vital manifestations with the structural appearance and invasion of other tissues. It had originally, and, indeed, up to a recent period, presented all the vital characters of a simple growth—very slow progress during twenty years, no pain, mobility and definition, and absence of any constitutional cachexy or disturbance; but its structure when cut into presented at one part all the appearance to the naked eye of

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"25th.—Patient has had less cough, but complains of dryness of his mouth. The tongue is moist and not much furred. Pulse has fallen to 108, and is of fair strength. Diet, milk with white of egg, beef-tea, and brandy. Quinine in doses of 1 gr. with dilute sulphuric acid in water to be given thrice a day.

"27th.—The cough has again been severe, otherwise the condition of the patient is satisfactory. The discharge is very small in quantity, and has no odour. Some stitches were removed

to-day, and the greater part of the incisions corresponding to the back of scapula seem united by first intention. There is a little irritation and discharge at the wound over the section of the clavicle, and the head of the humerus has a tendency to press against the cicatrix at the upper part, causing it to break up.

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medullary growth, and it had not only involved part of the capsule of the shoulder-joint, but had also infiltrated the neighbouring muscles and axillary glands which were removed.

These conditions were at variance with all my past observation of tumour growth. A very considerable experience had led me to lay down as an axiom that when a tumour was defined, of slow growth and possessed of the other manifestations of simple tumours, we might safely reckon on its maintaining that benign character, however much it might alter locally—that is to say, such a tumour, though it might ulcerate and adhere to other textures in consequence of local irritation, would not invade other tissues as malignant growths do; nor would it affect the constitution with true cachexy, and might be safely removed without risk of reproduction. In this case, however, the section of the tumour, the involvement of other structures, and more especially the infiltration of the long head of the triceps and other muscles, and the neighbouring chain of glands, so impressed myself and others who saw it that, on exhibiting the tumour to the Medico-Chirurgical Society, I expressed myself as rather shaken as to the views I had long held by what seemed so marked an exception to what I had hitherto observed. Subsequent microscopic investigations, however, would seem rather to confirm the axiom I have long held, for neither in the medullary-looking part of the tumour nor in the altered axillary glands was there any appearance of cancer cells.

The trouble experienced in the after treatment of the wound in this case has led me to reconsider the method of operating, and whether that might not be modified with advantage. The troublesome part of the treatment was the difficulty of obviating the constant tendency of the head of the humerus to be protruded at the upper part of the wound. Whilst the large posterior lines of incision corresponding to the body of the scapula healed almost entirely by the first intention, that part of the wound which corresponded to the outer end of the clavicle and acromion process was prevented from healing, and the cure rendered tedious by the pressure of the head of the humerus, and also to some extent by the irritation caused by the edges of the section of the clavicle, and when this part of the wound did heal, the head of the humerus was adherent to the cicatrix (*vide* Plate XIX.)

In the special memoir of a successful case of excision of the scapula published by the late Professor Syme no such difficulty is mentioned, but the reason is obvious. In Mr. Syme's case the



Mr. SPENCE on Excision of the Scapula.



head of the humerus had been excised some time previously on account of disease, and hence both the operation and after treatment of the wound would be simplified.

At first sight it may seem that there would be little difficulty in obviating the protrusion of the head of the humerus. That by merely attending to position, keeping the arm at a right angle with the trunk, or elevating it so as to depress the head of the humerus, the object in view would be attained. Such was my own opinion at first, but when I tried to put these plans into practice I found that they did not answer my expectations. When the arch formed by the acromion and clavicle is removed and the scapula excised, the cavity of the axilla ceases to exist, and the head of the humerus, however placed, is close to the surface, and has a tendency to be drawn or pushed upwards by the action of the pectoralis major. Moreover, it was found impossible to retain the arm in the positions indicated for any length of time. The only method that had any effect, and that very slight, was fixing the arm by bandaging it to a broad roller fastened round the pelvis.

Resection of the head of the humerus in such circumstances is a very obvious method of getting rid of the difficulty, and Mr. Syme's case is an instance in point; but unless the part was implicated by the disease, I think it would be unadvisable to add to the risks of the operation by such complication if we can attain our object otherwise.

The method I would adopt now would be to disarticulate the clavicle from the scapula, instead of sawing it through. By this means we would avoid a source of irritation produced by the section of the clavicle, and at the same time by leaving the broad flat acromial extremity of that bone we would have a *point d'appui* with which the head of the humerus could be kept in contact, and so prevented from protruding, and there a false or new joint might ultimately be formed.

Such a procedure might render the operation in some respects more difficult, because by sawing through the clavicle, as in the case narrated, the operator can most readily expose the great vessels so as to enable the assistant to command them thoroughly, and he can also most easily expose and deal with the muscles attached to the coracoid process; whereas in disarticulating the clavicle from the scapula he requires to divide both the acromio and coraco-clavicular ligaments from above and behind after

separating the attachments of the trapezius, and to avoid division of the integuments below the clavicle. Practically, however, I believe the difficulty could not be great, for I have on one occasion excised the scapula by this method in a case in which, after removing a large medullary cystic tumour from the axilla, I found there was a medullary growth from the subscapular fossa. In that case, owing to the exhausted state of the patient, I had to proceed very rapidly, and on the spur of the moment I divided the muscles attached to the base of the scapula and the trapezius, threw forward the scapula and arm, divided the coraco-clavicular ligament, separated the clavicle from the acromion, and removed the scapula after disarticulating the shoulder-joint. In Dow's case I adopted what I considered the more methodical plan of operating to secure against hæmorrhage, as already mentioned; but I believe that important object could be equally attained by direct compression above the clavicle through the incision necessary to separate the trapezius muscle, whilst the extent of the wound would be diminished, its after treatment simplified, its healing expedited, and a more useful arm left.*

ART. XV.—*Note on the Therapeutical Uses of Iodoform.* By H. S. PURDON, M.D., L.R.C.P., Physician to the Belfast General Hospital and to the Hospital for Diseases of the Skin.

DURING the last three years iodoform has been brought prominently under professional notice in France and United States. I have frequently used it with benefit to my patients, so I offer no apology for drawing the attention of my Irish brethren to this medicinal agent.

Iodoform possesses not only tonic and stimulant, but also alterative properties; the dose is from one to three grains given in form of pill. Externally it may be applied as an ointment in the

* NOTE.—April 11, 1873.—In my remarks on Dow's case I drew attention to some anomalous conditions in the clinical history, vital manifestations, and the general appearances and microscopic character of the tumour. I trusted that the subsequent history of the patient would assist in elucidating the true nature of the growth. Unfortunately, however, the patient, who, as stated in the report, was subject to bronchitis and cardiac disease, died last autumn from an attack of double capillary bronchitis; so that no reliable deduction can be drawn from the absence of any tendency to reproduction of the growth during the few months which elapsed after his recovery from the operation.—J. S.

proportion of one drachm of iodoform to the ounce of lard, a few drops of rectified spirit being first added to dissolve the iodoform. This ointment will be found useful to relieve the pain of cancerous sores; fissures, especially of the rectum; ulcers; to allay excessive pruritus of the skin, as in prurigo and scabies. For a vaginal suppository iodoform has been recommended by Doctor E. Cutter, of Boston, U.S., in painful uterine diseases. When introduced into the rectum as a suppository it exercises upon the sphincters a local anæsthetic effect, so that defecation has been performed unconsciously. In hæmorrhoids and tenesmus it is useful. Moûtre's formula is:—Iodoform powdered gr. xx., cocoa butter ʒi., melt and mix into six suppositories. As a disinfectant its power has been asserted by Righini. Certainly the odour is powerful and permanent, and may be compared to that of saffron. For ulcers the iodoform ointment is serviceable, especially when they are painful and indolent. Moreover, Doctor Izard has experimented with iodoform in the treatment of venereal ulcers: such sores it heals and cicatrizes rapidly. According to the same author, if iodoform be sprinkled upon a soft chancre it heals quickly and without pain. Moreover, it arrests the progress of phagedæna. M. Demarquay thinks highly of this remedy, whilst Doctor Stiles recommends it as an addition to the ordinary plasters and ointments for syphilitic periostitis. Doctor Gamberini (*Lancet*, April 12th, 1873), at the hospital of Saint Opsola, Bologna, treats all cases of soft chancres with iodoform, two drachms and a half to an ounce of glycerine. This remedy he has found highly successful. For burns, scalds, and painful boils the application of an ointment, containing thirty to forty grains to the ounce, is recommended as a good anæsthetic. Internally I have found a grain of iodoform with two grains of reduced iron given in form of pill twice or thrice daily a most excellent remedy for neuralgic and chronic rheumatic affections. In scrofulous complaints, such as glandular enlargements, it may be prescribed both locally and constitutionally, and its benefit will not be considered surprising when we remember that iodoform contains ninety per cent. of iodine. I am at present attending a young lady aged sixteen years, of prepossessing appearance, who consulted me for a glandular enlargement on side of neck, and which I need scarcely say disfigured her appearance. The usual routine treatment had been tried, such as iodine and its various preparations, &c. I prescribed iodoform and iron and iodoform ointment; the affection

is now rapidly disappearing. The following are some of the diseases in which I have given iodoform a trial, and that an extensive one. At the Belfast General Hospital I have ordered it in several cases of phthisis in the following way:—Iodoform sixteen grains, essence of aniseed one drachm, cod-liver oil eight ounces, mix; dose, a tablespoonful twice daily one hour after food. Also in neuralgia given with iron, in chronic rheumatism, chlorosis, anæmia, and as a suppository in the vagina in cystitis in the female. At the Belfast Hospital for Diseases of the Skin iodoform has been prescribed locally in prurigo, in pruritus ani, in cancerous ulceration and painful ulcers of limbs. In phthisis the remedy in second stage of the disease checks the night sweats, and relieves the hacking cough. Moreover, the cod-liver oil agrees better with the stomach when thus combined. In neuralgia it is often curative when other remedies have failed, and I can specially call to mind the case of Serjeant D——, Royal Irish Constabulary, as a case in point. It acts better when combined with reduced iron.

Although not to the point, it may be interesting to conclude this brief notice by remarking that Dr. Dupre, of the Westminster Hospital, found in the course of his experiments on elimination of alcohol, that after six weeks of total deprivation from alcoholic drinks, and even in one individual who completely abstained from alcohol, the urine contains sometimes a substance which is not alcohol, but which presents the characters by which we generally recognize alcohol. This body passes over among the earliest products of distillation, gives acetic acid on being oxydized, reduces bichromate of potash when dilute sulphuric acid is present, and its aqueous solution has a lower density than water. It furnishes *iodoform*, and exists in the urine in a very small quantity.

ART. XVI.—*Practical Notes from Kilkenny County Infirmary.* By DR. ZACH. JOHNSON, A.M., T.C.D., F.R.C.S.I.; Licentiate, King and Queen's College of Physicians, &c., &c.

POPLITEAL ANEURISM SUCCESSFULLY TREATED BY
COMPRESSION.

THE treatment of aneurism by compression is a subject of such great importance that the publication of each successful case seems to be a matter of duty. On these grounds the following details are now made public. Although it forms but a unit in the statistics

of aneurismal compression, even one successful case is of importance in determining the value of this plan of treatment; and although those cases already recorded are so numerous as to have set the question nearly at rest, the addition of another cannot be amiss, if only to strengthen the evidence already accumulated; while the circumstances and successful issue of that under consideration are useful, not alone in aiding toward the arriving at a just conclusion, but in teaching the important lesson that we should not be discouraged by difficulties, or abandon the prosecution of any useful or promising expedient, because our first efforts are not crowned with immediate or complete success.

Michael Murphy, an agricultural labourer, aged about thirty-four, and of medium size and healthy aspect, was admitted into Kilkenny County Infirmary in February, 1872, on account of a popliteal aneurism of the left leg, which gave him great pain, and was increasing rapidly in size. He could not assign any cause for its formation. His occupations were only those common to his class, and he was not aware of any accident, injury, or exertion which could have conduced directly to its formation. Moreover, he was a man of temperate habits.

His attention was first drawn to it by dull persistent pain, stiffness of the knee, and inability to accomplish the ordinary motions of the joint. He applied for advice to the late Dr. Leech, of the Gowran Dispensary, who at once appreciated the true nature of the disease, and recommended him to apply for admission into the County Infirmary.

On admission a strongly pulsating tumour was distinctly observable in the left popliteal space. That it was not merely pulsation communicated by the underlying artery was plain from the expansile character of the pulsation, and by its total disappearance on obliterating the current of the femoral artery by pressure on the ramus of the pubis.

The nature of the case being sufficiently plain, it was at once decided to give a fair trial to the method of compression. Accordingly, the patient was confined to bed. Two clamps were applied, so as to enable him to relieve the pain arising from continued pressure by their alternate use. By means of the ordinary large pelvic instrument, with combined movements, and ball and socket joint, pressure either above or below Poupart's ligament was provided for, while Lister's more simple instrument, embracing the thigh, effected alternate pressure in the triangular

space comprised between the sartorius, adductor longus, and Poupart's ligament.

These instruments were fully capable of arresting the arterial current, and stopping pulsation in the tumour, if only the patient could have been prevailed on to endure their alternate use. But he proved *extremely intolerant* of all pressure, and did not carry out the directions given him; it has been since ascertained that he invariably relaxed the instruments as soon as his attendants turned their backs. The tumour increased with great rapidity, and the pain became more and more intolerable. In a short time the appearances became very formidable and alarming. The swelling was no longer confined to the popliteal space, which was now greatly enlarged, and the entire knee became distended by synovitis, which supervened gradually. The pulsation became daily more violent and more widely diffused, the tumour encroached rapidly on the inner side of the joint, a dusky shade overspread the greater part, and over the inner condyle of the femur a patch became conspicuous, dark and well-defined, about the size of a florin, with well-marked line of demarcation, indicating unmistakably that a slough was on the point of forming, and that in a few days, perhaps hours, the tumour would burst, and the patient perish, unless prompt and effectual means were taken to arrest the progress of the disease.

However, previous to resorting to the operation of tying the femoral artery, a measure attended with some danger, and, at the best, uncertain in its result, it was determined to give one final trial to compression by means of continued digital pressure.

The measurements at this time, March, 1872, were as follows:— Sound leg, girth round patella and popliteal space twelve inches; diseased leg, corresponding measurement seventeen inches: sound leg, girth of thigh immediately above patella eleven inches; diseased leg, corresponding measurement sixteen inches: longitudinal measurement of tumour four inches.

These measurements indicate a very considerable enlargement, but the critical position of the case was more forcibly shown by the suggestive dusky complexion of the tumour, the defined spot of impending sphacelus, the violent pulsation, which was hourly acquiring greater force, and the pain, which had become intolerable, and for the relief of which anodynes had now become ineffectual.

Arrangements were made accordingly, and a day fixed for the final trial of compression by persistent digital pressure, applied by

a succession of skilled assistants, who undertook to give an hour or more each in turn until some decided result might be obtained. From 8 sharp a.m. till 10.30 p.m., 7th March, 1872, effectual pressure was never allowed to relax for a moment, being applied alternately above and below Poupart's ligament, but principally on the ramus of the pubis, where the patient bore it best, and where a smaller amount of pressure had a more decided effect on the arterial current. About 1 o'clock p.m. the patient became almost uncontrollable from intense pain in the tumour, and struggled hard to break away, but was with difficulty restrained, partly by force, and partly by the earnest remonstrances of those present. And here material advantage was obtained by the interposition and personal influence of Sir John Blunden, Bart., one of the governors of the hospital, but for whose encouraging interference as an amateur, and reassuring remonstrances with the patient, the best efforts of the medical staff were in danger of being baffled.

For some time the patient struggled violently to break away, and it was with difficulty he could be kept in bed and pressure continued. However, by judicious restraint his struggles were overcome, and the pressure, which had never been permitted to relax, successfully continued. A full opiate was promptly administered, and preparations made for bringing him under the influence of chloroform, but before they were completed the opiate appeared to have taken effect, the intense pain abated, and the patient once more became amenable to treatment.

By the kind assistance of my colleague, Dr. Kinchela, aided by Drs. Lyster and Fitzsimons, and Mr. White, of Kilkenny Dispensary, each undertaking the maintenance of digital pressure by turns, effectual compression was unremittingly sustained from 8 o'clock a.m. until 10.30 at night, when it was gradually withdrawn, and it was then found, to the great satisfaction of those interested, that there was not a vestige of pulsation in the tumour.

The pain in it had never returned since 1 o'clock p.m., and the patient had borne the digital pressure throughout without inconvenience. The intolerable pain complained of at mid-day was in the tumour itself. He now got a full anodyne. He had been given aliment at intervals during the day while pressure was going on.

He had a good night, and from this out the progress of the case was most satisfactory. The dusky hue gradually left the tumour. The defined semi-sphacelated spot soon recovered its natural colour.

From day to day the tumour slowly diminished in size. No trace of pulsation ever reappeared. Health and general aspect rapidly improved. On the 10th July the measurement round patella was the same as that round thigh just above it, and had been reduced from seventeen and sixteen inches to fourteen and a-half. On the 27th August it had been further reduced to fourteen. He had now been for some time walking about, the only inconvenience remaining being inability to perfectly straighten the leg, and pain and stiffness in the instep.

He has since presented himself at the hospital able to straighten the knee perfectly; it has now attained its natural size. The tumour has been almost altogether absorbed, and only a small firm kernel is to be found in the popliteal space. He has been able to resume his ordinary duties as a field labourer, and is in excellent health.

So far as the circumstances of a single case are of value nothing could speak more favourably for compression than the one under consideration. It proves, at least, that before resorting to an operation which cannot be divested of danger and uncertainty by the most consummate skill, the surgeon is bound to give a fair and patient trial to compression. That it is not suitable to all cases, and that it will not succeed in all must be admitted; but that it ought to be fairly tried in all, when at all applicable, the past history of compression goes far to prove, and the preceding details are now offered as a contribution to the statistics already accumulated on this important question.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Principles of Animal Mechanics. By REV. SAMUEL HAUGHTON, F.R.S., F.T.C.D., M.D., D.C.L., &c. London: Longmans and Co. Pp. 495.

THE science of animal mechanics has attracted attention for many years, owing to the beauty and complexity of the problems which it offers, but this is the first time that it has been made the subject of a methodical treatise, and that an attempt has been made to formulate its general principles. Dr. Haughton has succeeded in founding an additional exact science, and his book will long occupy the chief place in its literature.

Some of the older anatomists had endeavoured to make the study of muscular action resolvable into mechanical propositions. Borelli, in 1680, published at Rome his great book, "*De Motu Animalium*," and Bartholin and Fabricius have done some little in this direction. In more modern times the brothers Weber—the one a professor of anatomy, and the other of mathematics, have been working in the same field. Henke also has contributed to the *Zeitschrift für Rationelle Medicin* a series of papers on the mechanism of joints, as also has Langer; and Prof. Aeby, of Bern, very nearly stumbled on the method that has been so successful in Dr. Haughton's hands, by using the weights of muscles as the means of calculation (see his paper, Siebold & Kölliker's *Zeitschrift für Wissenschaftliche Zoologie*, Band X., p. 34). The combination of anatomist and mathematician in the one person is uncommon, and no other need attempt to take the subject in hand, as it requires a high degree of ability to use geometrical analysis.

The work before us is interesting in three aspects—firstly, the mechanical; secondly, the anatomical; and thirdly, as bearing on the question of origin of species.

1. The determination of the absolute force of muscles is one of the first points with which Professor Haughton starts, and this he

has determined by experiments on the shoulder muscles in holding the arms weighted and outstretched, &c., and from the data of Weber's and Koster's experiments on the gastrocnemius, Henke and Knorz on the same muscles and on the flexors of the forearm, and from the known cross section of these muscles as given by Donders, Mansfeldt, and from Dr. Haughton's own measurement; this he determines to be 102 lbs. per square inch. From observations and experiments on this subject he has deduced the law of fatigue, which he enunciates thus:—When the same muscle (or group of muscles) is kept in constant action until fatigue sets in, the total work done, multiplied by the rate of work, is constant.

He has also stated as laws the following propositions:—

I. In comparing together different muscles, the work done in contracting is proportional to the weight of each.

II. In comparing the same muscle with itself when contracting under different external conditions, the work done is always constant in a single contraction. The germ of law No. I. exists in Borelli's book, hence Dr. Haughton has called it Borelli's law. The second is based upon experiments made independently by Mr. Stanley Jevons, of Manchester, and Dr. Haughton himself. The third law is that of fatigue, stated above, and which is founded on a large series of experiments by Jevons, Nipher, Dr. Haughton, and others. The fourth law is that of refreshment, being a calculation of the rate at which the muscle becomes fed by arterial blood so as to be fitted for its work, of which it has been deprived by fatigue. Defining the coefficient of refreshment as the work restored to the muscle in foot-pounds per ounce per second, Dr. Haughton obtains from repeated experiments 0·1309 as the coefficient in ordinary voluntary muscle. Curiously enough, the physical data of the heart's action give 0·2377 as its coefficient of refreshment, a number explicable only on the theory of the diastolic and systolic supply of the heart wall. Previous to stating these general laws, and after his determination of the coefficient of muscular contraction, Professor Haughton studies particular cases of muscular action as exemplified in the heart, uterus, &c., prefixing to them this postulate—"The Framer of the universe has constructed all muscles on the principle that each shall perform the maximum of work possible for it under the given external conditions;" and having studied these and other cases of muscular action, he shows that this postulate is true for them. As Dr. Haughton has, by the medium of this Journal, already put before the public his views on

the action of the heart and the uterus, there is no need for repeating them, or commenting on them.

Dr. Haughton then states a simple geometrical classification of muscles, of which he makes two great classes.

First, muscles whose fibres lie in the same plane. Of this there are three orders—A. fibres parallel and rectilinear (including α . direct prismatic muscles, as sartorius, cleidomastoid; β . rhomboidal muscles, as the intercostals and rhomboid; γ . penniform, as the biceps of ungulates); B. fibres intersecting and rectilinear (including α . triangular muscles, as the great pectoral; β . deltoidal muscles, as the trapezius; and γ . quadrilateral, as the gluteus maximus; C. sphincter muscles, which have curved fibres in the same plane.

The second class of muscles includes those that form curved surfaces, which may have rectilinear fibres (constituting skews) or curvilinear, producing ellipsoidal surfaces.

The discussion of this classification is one of the most important points in Professor Haughton's work. Thus he shows that there is neither gain nor loss in absolute inherent work in a penniform arrangement of fibres, but that there is a change in the disposal of the work. A penniform muscle gives a powerful pull through a short space, a prismatic muscle is less powerful but moves in a larger space. The consideration of sphincter muscles is a very clear, detailed study of the rather difficult mechanical arrangements of these muscles, and the investigation of the action of skew and ellipsoidal sheets of muscles is, we think, the best part of the entire book. In this study he first determines the action of muscles whose fibres are all in a plane as producing rotation of a fixed, on a movable, bone, and from the data of the muscles he determines geometrically where the centre of rotation—that is, the socket—should lie. In all cases the calculated and observed locus of the socket coincide. On this he bases a scholium by way of query. May not the remarkable similarity in the bones and muscles of various animals, in corresponding parts of their structure, be the result of some such mechanical necessity foreseen by the Divine contriver, instead of being the result of common descent from a remote ancestor possessing a similar arrangement of bones and muscles? In other words, on Professor Haughton's theory homology is a mechanical necessity flowing from similarity of function. The teleological bearing of the skew muscle is most important, and Professor Haughton has, therefore, with great care

worked it out, and he comes to the conclusion that a skew muscle is a "supplemental contrivance" tending to bring the resultant line of force higher up on the bone, in some cases effecting a transfer of 32 per cent.

The anatomical part of the study is of very great interest. Professor Haughton has given details of the structure of the tendons of the hand and foot in man and numerous other vertebrates, and he has also developed a theory of muscular types, *i.e.*, the percentages of the various groups of muscles round the joints. As Dr. Haughton has had an unusually extensive field from which to collect his data, this part of the work is of great value to the comparative anatomist, and is much more extensive and perfect than the corresponding investigations of Professor Aeby. Dr. Haughton thus works out the muscular types of the felidæ, canidæ, primates, struthionidæ, bird's wing, &c.

In discussing the primates, Professor Haughton makes a statement which may be called in question, that "the difference between man and the gorilla is greater than the differences between the quadrumana compared with each other." Now, we must premise, firstly, that the data are scarcely comparable, for Dr. Haughton's chimpanzees and gorilla were under three years old, while his human subjects were adults. But even admitting Dr. Haughton's data as indicating relations absolutely and universally unalterable, the figures can bear another interpretation. They are as follows:—

Percentages of Muscles and Hip-joint.

	Abductors	Flexors	Extensors	Adductors
Man, - - -	39·67	18·21	16·65	25·44
Gorilla, - - -	33·04	13·12	24·12	29·72
Chimpanzee, - - -	32·50	17·92	20·83	28·75
Hamadryas Baboon, - - -	26·42	18·00	34·59	20·99
Mandrill, - - -	22·77	14·99	34·72	27·52
Ateles, - - -	20·84	21·60	25·79	31·77

The simplest way of stating these relationships is as follows:—

	Abductors	Flexors	Extensors	Adductors
Man = Gorilla,	+ 6·63	+ 5·09	- 7·47	- 4·28
Man = Chimpanzee,	+ 7·17	+ 0·29	- 4·00	- 1·00
Chimpanzee = Man,	- 9·73	+ 2·93	- 13·47	+ 4·28
Chimpanzee = Ateles,	- 6·66	- 3·65	- 4·90	+ 2·73
Chimpanzee = Hamadryas,	- 6·00	- 6·08	- 13·76	+ 1·75
Ateles = Hamadryas,	- 3·16	+ 3·60	- 5·80	+ 2·00

A consideration of this table will show that there is less difference in muscular type between man and the chimpanzee than between any two others of those compared, and as little difference between man and the gorilla as between any of the individuals in the series.

The general conclusion to be drawn from the book is, that in the animal kingdom every working mechanism is mathematically perfect for its function. The acute mind of Professor Haughton, however, has foreseen that this conclusion is equally comprehensible on the evolution theory as on the creation hypothesis, and the Professor cannot see why there may not be in organic life an evolution of higher from lower forms of existence. He only postulates that this evolution has been one under supervision, and this point no one who examines the facts stated in the book can honestly doubt. The statement that homology is a teleological necessity in parts with similar actions, is one which does not touch on what is vital ground in homology—namely, rudimental organs. No teleological necessity can possibly govern the accessory metacarpals of the horse, the tendonless, accessory toes of the deer, or any of the thousands of rudimental organs in the animal kingdom. Professor Haughton has himself in his studies been brought face to face with the difficulty of rudimental organs in the case of the wings of the struthionidæ, and he postulates that they were never intended for flight; but a study of his own figures will show that the struthious type is that to which a wing when disused tends. Thus in his tables, pp. 422-8, he quotes data as follows:—

	Scapular Muscles	Abductor	Flexor	Extensor	Adductor
Rhea, . . .	8.53	45.50	21.09	8.06	16.82
Weka Rail, . . .	10.43	29.56	13.04	6.95	40.02
Dorking Fowl, . . .	4.70	28.35	5.38	3.11	58.76
Curassow, . . .	2.58	26.29	6.50	4.18	60.45
White Crane, . . .	7.16	23.88	7.69	5.30	55.97
Gannet, . . .	5.51	13.07	11.65	1.26	68.51
Grebe, . . .	2.05	13.93	6.97	1.23	75.82

Read comparatively, this table is as follows:—

	Abductor	Flexor	Extensor	Adductor	Scapular
Weka Rail, . . .	-29.90	-0.12	-1.69	+19.89	+2.82
Dorking Fowl, . . .	-22.11	-7.73	+5.53	+38.63	-3.21
Curassow, . . .	-27.17	-6.66	-4.46	+40.32	-5.03
White Crane, . . .	-26.58	-5.47	-3.34	+35.84	-0.45
Gannet, . . .	-16.49	-1.39	-5.69	+28.49	-4.92
Grebe, . . .	-15.63	-6.07	-5.72	+35.80	-8.38

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Chimpanzee=Ateles, . . .	+11·66	—3·68	— 4·96	
Chimpanzee=Hamadryas . . .	+ 6·08	—0·08	—13·76	
Ateles=Hamadryas, . . .	— 5·88	+3·60	— 8·80	

PART III.

HALF-YEARLY REPORTS.

REPORT ON PUBLIC HEALTH.*

By CHARLES A. CAMERON, Ph.D., M.D., L.K. & Q.C.P.;
Prof. of Hygiene in the Royal College of Surgeons; Lecturer
on Chemistry in Steevens' Hospital Medical College, and the
Ledwich School of Medicine; Analyst to the City of Dublin, &c.

ADULTERATION OF ALCOHOLIC LIQUORS.

DURING the last Session of Parliament an Act was passed, commonly termed the "Licensing" Act, regulating the sale of intoxicating beverages. This Act contains a schedule of articles, such as cocculus indicus, grains of Paradise, bluestone, &c., which are prohibited from being mixed with spirituous liquors, under heavy penalties. There appears to be a general belief in this country that almost every kind of alcoholic beverage, but more especially whiskey, is largely and systematically adulterated. The popular notion is that all the whiskey sold at fairs, in shebeens, and low public houses, is stuff compounded of a little whiskey mixed with a large proportion of water, strengthened by oil of vitriol, bluestone, and similar substances. Some people go so far as to assert that liquids are palmed off upon us under the name of whiskey which are altogether destitute of the pure spirit.

We have reason to doubt that whiskey is, except very rarely, adulterated at all; and we are quite certain that the wholesale sophistication of this liquid, generally believed to be the case, has no existence save in the popular fancy. We have examined from time to time a very large number of specimens of whiskey, and we have not as yet met with an adulterated one. We mean by the

* The author of this report will be glad to receive any books, pamphlets, or papers relating to hygiene, dietetics, &c. They may be forwarded through the agencies of this Journal.

term adulterated an article to which a cheaper substance had been added for the purpose of fraudulently increasing its weight or volume. We have tested whiskey procured at fairs, seized upon by the police at night houses, supplied to public institutions, and bought by private persons at public houses and groceries of all degrees of respectability, but in no single case did an adulterated specimen come under our notice.

Although there is little, if any, adulterated whiskey to be met with, there is abundance of weak and bad spirits on sale in Ireland. Whiskey is always sold from 10° to 25° over proof by the distiller, but the retailer generally vends whiskey at from 5° to 30° below proof. The Act for the prevention of adulteration, whilst it renders the addition of water to milk an illegal act, allows the "reduction," i.e. dilution, of spirits to be performed at the discretion of the dealer. In the Isle of Man a recent Act of the House of Keys—as the local legislature is termed—prohibits the reduction of strong alcohols to any degree lower than 11° under proof, unless the strength is specified.

When whiskey is very weak the consumers of that article amongst the lower classes do not care to drink it unless it is tolerably new. Old whiskey is mild, and if it be at the same time, poor in alcohol, there would not be, to use a common phrase, sufficient "bite" in it to satisfy the strong palate of the agricultural labourer, or of the navy.

During the fermentation of the grain intended to be used in the preparation of whiskey, two species of alcohol are formed, namely, ethylic, or vinic alcohol, and amylic alcohol. The former boils at 173° Fahr., the latter at 269° Fahr., therefore they can readily be separated by careful fractional distillation. The first portion of the whiskey which passes over during the process of distillation is very free from amylic alcohol; but the last portion of the distillate, technically termed "faints," is mixed with a large but variable amount of amylic alcohol.

Amylic alcohol, or fousel oil, as it is commonly termed, possesses a most acid burning flavour and an oppressive odour. A very minute trace of it in ethylic alcohol greatly injures the flavour and odour of the latter. If the fousel oil is removed from whiskey by rectification, with it are also taken out various ethers of the acids of the acetic series, and the whiskey is converted into common spirit of wine, which few would care to substitute for whiskey or brandy, notwithstanding its purity. The fousel oil and the

substances associated with it must therefore be allowed to remain in the whiskey until in the course of time they are converted into those compounds of agreeable odour and flavour which characterize old whiskey. Those who relish a fiery whiskey do not object to that which contains a large amount of fousel oil; on the contrary they regard with suspicion and ill favour a glass of Jameson's five years old mild whiskey.

New whiskey is much cheaper than old, and the bad flavour (to refined palates at least) of the former is often attempted to be disguised by the addition of flavouring materials. For this purpose the following materials are used:—Sherry wine, tartaric and acetic acids, sugar, pine apple, and other fruit essence, tincture of prunes, acetic ether, oil of wine, spirit of nitrous ether, glycerine, green tea, and various other substances. A mixture of burnt sugar, sherry, acetic ether, and tartaric acid, is very commonly used to "convert new whiskey into old." These substances are used in very small quantities; and it is doubtful if they ever produce any bad effects upon the animal economy. They, however, are objectionable, because they are often the means of inducing people to drink whiskey largely contaminated with fousel oil.

Amylic alcohol is almost deserving of the name of rank poison. A small quantity of it in whiskey renders the latter an unwholesome liquor; and the bad effects which we constantly hear attributed to the use of adulterated whiskey are undoubtedly due to the use of new whiskey largely contaminated with fousel oil. The sale of such whiskey should be prohibited. For this purpose it would only be necessary to prevent the removal of spirits from the bonded warehouse, until after at least one year's storage. We have reason to believe that whiskey is sold in country districts almost immediately after its manufacture.

In the Fourth Annual Report (for 1872) of the State Board of Health of Massachusetts, there is an interesting report in which the subject of the flavouring of articles of food and drink is almost exhaustively treated. It would appear that in the United States such substances as acetate of amylic ether and alcoholic solution of butyric ether are largely used for the purpose of imparting factitious odours and flavours to various confections and beverages. "Strawberry," "raspberry," and other fruit jellies are made without the assistance of any fruit. They consist of gelatin and sugar, flavoured with "essences." The latter are alcoholic solutions of ethers, tinctures of orris root, vanilla, &c. The amount of these

flavouring matters in confections and beverages is very small; but it is possible that their continued use may, especially in the case of children, be more or less productive of mischief.

The oil of bitter almonds is extensively used as a flavouring ingredient of food and drink. It often contains a decided amount of prussic acid, and fatal accidents have occurred from an excessive amount of impure oil being used in the preparation of custards and puddings. Even bitter almonds themselves have on two or three occasions, when eaten in large quantities, been the cause of death. Nitro-benzole (a substance resulting from the action of nitric acid upon benzole, a hydro-carbon derived from coal tar) appears to be used as an adulterant of oil of bitter almonds in the United States, and probably in these countries too. It is a deadly poison, and several deaths from very small doses of it are recorded in the *Boston Medical and Surgical Journal*, January 18, 1872. It is probable that owing to its cheapness it may be occasionally substituted for oil of bitter almonds as a flavouring ingredient of foods and beverages.

PUTREFIERS AND ANTISEPTICS.

Dr. John Dougall, of Glasgow, is continuing his interesting experiments in relation to the power of certain substances to check decay, and to destroy the lowest forms of life. In the *Glasgow Medical Journal* for November, 1872, and February, 1873, the results of his latest investigations are recorded. His general method of procedure consists in adding determinate quantities of salts, acids, alkalies, and various other substances to measured quantities of some such organic mixture as filtered solution of beef juice, or infusion of hay. The mixtures are kept at a temperature of from 58° to 68° Fahr., and examined carefully periodically. The appearance presented by each mixture after the lapse of a certain period of time is taken as showing the efficacy of the antiseptic employed. In some cases the beef juice begins to putrefy very rapidly, and large numbers of low forms of animal or vegetable life, or of both, are observed in it. On the other hand when mixed with certain antiseptics, organic matter remains for a very long time without putrefying.

Dr. Dougall considers that the relative efficacy of the substances with which he experimented as anti-putrefactive and anti-fermentive agents is approximately shown in the following table:—

Name of Group	Antiputrefactive Power	Antifermentive Power	Total Antiseptic Power
I. Metallic Salts, - - -	46·6	64·1	110·7
II. Organic Acids, - - -	42·1	43·6	85·7
III. Inorganic Acids, - - -	35·6	37·7	73·3
IV. Salts of the Alkaline Earths, -	29·6	41	70·6
V. Organic Salts, - - -	12·7	44·4	57·1
VI. Tincture of Iodine and Alcohol,	5·3	76·5	81·8
Totals, - - -	171·9	307·3	

Dr. Dougall considers that the results of his experiments fairly admit of the following conclusions:—

“1. *That Putrefaction and Fermentation are not identical processes, though apparently producing similar results.*—Putrefaction *per se* in a fluid is characterized, as already stated (*vide* putrefiers), by the presence of myriads of microzymes, putrid odour, haziness, neutral, alkaline, or faint acid reaction and slowness of change, as compared with that produced by fermentation. Fermentation *per se* in a fluid consists in the presence of torulæ, tufts of mycelia, leptothrix filaments, and other cryptogams, mouldy aroma, transparency of fluid, distinct acid reaction and rapidity of change, as compared with that produced by putrefaction. These processes, however, though obviously different, apparently yield similar results. When consummated, the organic molecules, according to all observers, are broken down into simpler compound units: the microzymes, fetor, haziness, fungi, and mouldy aroma disappear, while the liquids hitherto responding to the tests for albumen cease to do so. Though these results, however, resemble each other, perhaps were the residual fluids submitted to fractional distillation they would yield products characteristic of the change the solutions had undergone from which they were derived.

“2. *Simple Solutions of Organic Matter, when they decompose, Putrefy in the first instance.* *Vide* Simple Solutions and 5. This is so well known that to mention it may seem superfluous, were it not that it also expresses its antithesis.—Simple solutions of organic matter, when they decompose, do not ferment in the first instance. The proximate cause of this is the neutral reaction of the solution.

“3. *Different Organic Solutions vary in the time of their Decomposition.*—This is well illustrated in the following table, where the number of days and of blanks occurring in the animalculæ and fungi columns in the separate solutions are exhibited.

Beef Juice				Beef Juice, Urine, and Egg Albumen				Egg Albumen			
Animalcules		Fungi		Animalcules		Fungi		Animalcules		Fungi	
Days	Blanks	Days	Blanks	Days	Blanks	Days	Blanks	Days	Blanks	Days	Blanks
721	4	612	6	678	6	428	12	767	8	355	23
1121		1212		1278		1628		1567		2655	

"Assuming each blank to represent one hundred days, the above totals are obtained, showing that while the three solutions vary in the time in which they decompose, there is yet a remarkable gradation and harmony of results. For example, the total animalculæ days in beef juice is 1121; in beef juice, urine, and egg 1278; and in egg albumen 1567. In the same order we have the total fungi days: thus, 1212, 1628, 2655; or, if the totals be read as they occur in the table, it will be seen there is almost a consecutive increment from first to last. These figures prove that beef juice is the most, and egg albumen the least, prone to change of the three solutions.

"4. *Putrefaction is more difficult to prevent than Fermentation.*—This is seen in the table of approximate averages where putrefaction is found to ensue in 172, and fermentation in 307 days. Again, in the table appended to 3, in the antiputrefactive columns there are only 18 blanks, and in the antifermentive columns 41 blanks.

"5. *The majority of Neutral and faintly Acid Solutions of Organic Matter, when they Decompose, Putrefy in the first instance.*—Numerous examples in the tables prove this. In the few exceptions fermentation has immediately or soon merged into putrefaction.

"6. *Neutral Mixtures remain Neutral after Putrefying.*—This statement has only one exception—pure quinine in the third fungi column is alkaline.

"7. *Fermentation and Putrefaction may proceed for a short time together, but almost never originate simultaneously.*—The only exception here is alcohol, where, in the beef juice columns, it will be seen animalculæ and torulæ appear on the same day (fourth).

"8. *Fermentation very frequently subsides into Putrefaction, but Putrefaction is seldom intensified into Fermentation.*—Out of 65 instances in which fermentation and putrefaction occurred in the same solution, fermentation changed into putrefaction 55 times, and putrefaction into fermentation only 10 times. In the former the change took place early, generally in from two to ten days. In the latter, late, in from 50 to 90 days. In the ten instances torulæ were present only, and sparse, except in the alcohol

solution, in which were penicillia only. Hence, when fungi appear in a fluid which has putrefied they are almost always torulæ.

"9. *When Penicillia are found in a Solution of Organic Matter it is almost always Acid.*—In the 47 solutions in which penicillia occur, 42 are acid and only 5 neutral. Those in the acid solutions were greatly more exuberant than the others. This peculiarity characterizes the whole fungus tribe, its habitat being a soil of pure vegetable mould, such as decayed wood, which, according to Mulder, contains geic, humic, and ulmic acids. The juices also of fungi are acid. The boleti yielding oxalic, or, according to some, boletic acid, while other species, as the agarici, &c., contain fungic acid, some say also hydrocyanic acid. For some time past I have tested the reaction of a great many fungi, mucors included, by bruising them on litmus paper, and found it always reddened.

"10. *Moderately Acid Solutions of Fresh Organic Matter generally first quickly Ferment and soon Putrefy.*—This is so in 50 of the 81 acid mixtures recorded. The exceptions have neither fermented nor putrefied, as mercuric dichloride, benzoic acid, &c.; or putrefied only, as sulphurous and hydrochloric acids, or fermented after putrefying, as tartrate of antimony and bisulphite of lime, or fermented without putrefying, as sulphuric acid, picric acid, and potassic dichromate. Hence fermentation or putrefaction may be produced at will.

"11. *Most Fresh Organic Fluids rendered acid, becoming Putrid, become Neutral.*—This law has very few exceptions—eight out of 81 instances. Zinc chloride is a prominent one, being putrid and acid at once in its three solutions.

"12. *The same Acid substance in different proportions may determine Putrefaction or Fermentation.*—Though I have strong reason to believe this applies to most of the acid substances, yet only one example can be given. In the egg albumen column, Group IV., under chloralum (aqueous solution of aluminic chloride) animalcules appeared on the ninth day, and under aluminic chloride, fungi on the eighth day; the weaker solution determining putrefaction and the stronger fermentation.

"13. *The Odours of Fermentation and of Putrefaction are distinct.*—Organic matter in a state of fermentation has usually an earthy or mouldy aroma, frequently not unpleasant, sometimes, indeed, slightly odoriferous. I have observed milk, after being exposed to the vapours of various volatile media, strewed with woolly and fawn coloured tufts of mucedinæ, and having a slight odour of decaying rose leaves. Moreover, as already noticed, the citric acid and beef juice solution, during its fermenting stage, gave out a faint orange perfume. The effluvia of putrefaction vary greatly, but their mildest forms are repulsive, and usually characteristic of their source. A putrescent taint is *sui generis*, and easily discriminated from the musty rotten-straw odour of fermentation. The peculiar nauseating fetor of pus from necrosed bone is a well known instance, and

is, doubtless, owing to the abundant presence of lime salts, chiefly the tribasic phosphate. When caustic lime (see putrefiers) and chloride of lime were used, the odour of their putrid mixtures at once recalled that of osseous suppuration.

"14. *Almost all Antiputrefactives are Acid.*—A slight glance at the tables will show the truth of this axiom. Indeed, it may be safely asserted that marked putridity and acidity conjoined is a chemical impossibility. Much more so is 'strongly acid and weakly putrid.' The converse, however, 'strongly putrid and weakly acid,' is possible but transient, the mixture soon getting neutral, and, in fact, was the condition of those solutions in the tables forming the exceptions.

"15. *The best Antiputrefactives are Acid.*—*Vide* mercuric dichloride, chromic acid, potassic dichromate, cupric sulphate, benzoic acid, argentic nitrate.

"16. *Good Antifermentives are commonly bad Antiputrefactives, and are Neutral.*—*Vide* baric chloride, quinine (pure), beberia sulphate, iodine, alcohol.

"17. *A good Antifermentive may be a fair Antiputrefactive.*—*Vide* zinc chloride, sulphurous acid, hydrocyanic acid, quinine, iodine.

"18. *Bad Antifermentives are commonly good Antiputrefactives, and are Acid.*—*Vide* aluminic chloride, sulphuric, oxalic, nitro-hydrochloric, and arsenious acids, plumbic acetate, &c. To illustrate more fully the truth of this axiom, compare the figures of aluminic chloride, when it will be seen that this body has manifestly caused fermentation, as, in its absence, all its mixtures being then mere simple organic solutions, would undoubtedly have putrefied. *Vide* 2. Hence, if fermentation be an evil in a pathological sense equal to putrefaction (some think it worse), the use of this body and others of a like nature as antiseptics is either obviously useless or positively injurious.

"19. *A good Antiputrefactive may be a fair Antifermentive.*—*Vide* potassic dichromate, iron alum, zinc sulphate, cupric sulphate, plumbic acetate, picric acid, &c.

"20. *Fermentation may begin and end per se in an Organic Solution.*—*Vide* all mixtures figured in the fungi columns and blank in the corresponding microzyme columns.

"21. *Putrefaction may begin and end per se in an Organic Solution.*—*Vide* all mixtures figured in the microzyme columns and blank in the corresponding fungi columns.

"22. *The best Antiseptics are Acid.*—*Vide* mercuric dichloride, benzoic acid, chromic acid, cupric sulphate, argentic nitrate, potassic dichromate. An antiseptic is a substance which prevents synchronously fermentation and putrefaction of organic matter. If it but do the first, it is merely an antifermentive, if only the second, it is simply an antiputrefactive. A body may seem to have a high antiseptic power in regard to preventing

fungi, because it has a low antiputrefactive power in regard to preventing microzymes, as where putrefaction is strong fungi seldom appear, or only sparsely and at long intervals. The converse also obtains, though not to such an extent. A body may seem to have a high antiseptic power in regard to preventing microzymes because it has a low antifermentive power in regard to preventing fungi, as where fermentation is strong microzymes may not appear, though they generally do in abundance in comparatively short intervals."

Dr. Dougall also gives us the results of experiments which he made to determine the influence of antiseptics and disinfectants on vaccine lymph. He states that his results, so far as chlorine is concerned, confirm our own experiments recorded in this Journal, and which proved that the virus of the lymph resisted the influence of chlorine gas, except when applied in very large quantities.

P. C. Plugge has written a very elaborate paper* on the value of phenol (carbolic acid) as a disinfectant. The author inclines to the view that living organisms originate the process of fermentation and putrefaction. He considers that in any case it is important to render the organisms that are found in putrefying and fermenting substances harmless. He finds that in liquids sufficiently acidulated only harmless moulds appear; and therefore he proposes to add an acid to organic liquids, so as to prevent the production of dangerous forms of animal life. Phenol, according to the author, is the most efficacious acid to employ. The butyric fermentation in milk was prevented by the addition of $\frac{1}{100}$ part of phenol, and minute quantities of it prevented the transformation of starch into sugar, and of albuminoids into peptones. The antiputrefactive influence of phenol was compared with sulphate of iron, chlorine, chloride of lime, the permanganates, and the mineral acids; and in every instance the result was in favour of the carbolic acid.

The researches of Fremy (*Les Mondes*, No. 7, 1872) led him to assert, contrary to the views of Plugge and many others, that fermentation can take place without the presence of living organisms. He considers that he has produced alcoholic, lactic, and butyric fermentation in the complete absence of germs. He claims to have demonstrated by experiments upon barley, grapes, milk, beer, yeast, and the mother liquors, that ferments are not derived from germs.

* Pflueger's Archiv. f. Physiologie, v. 538.

VACCINO-SYPHILIS.

At a meeting of the Royal Medical and Surgical Society, held at London on the 28th January, 1873, Mr. Jonathan Hutchinson read his second report upon the communication of syphilis in the practice of vaccination. The cases which he records certainly strengthen materially the statements put forth in his first report upon this matter. The following is a brief history of the cases detailed in the report. A respectable married man, aged forty-six, applied to Moorfields Ophthalmic Hospital on account of iritis, which at once was perceived to be syphilitic. He had been vaccinated three months previously, but the sore which healed readily broke out a month after vaccination, and assumed the form of a hard chancre. A fortnight later an abundant secondary rash appeared; and in a month after iritis set in. This man had been vaccinated with lymph taken from a child who showed no symptoms except "a sunken bridge of the nose." Twelve persons had been vaccinated from lymph obtained from this child; and there was reason to believe that the man in question had been the last. It is therefore probable that in his case the lymph was mixed with serum or blood. The second case recorded by Mr. Hutchinson was that of a lady, aged forty-five years. She consulted Mr. Hutchinson as a private patient, and complained of a vascular growth in the urethra. During examination the remains of a copious and dusky rash, evidently of syphilitic origin, were discovered. She stated that five months previously she had been vaccinated by four punctures, none of which proved successful. A severe rash and inflammation of an eye followed, and she felt very ill. A careful examination showed the presence of a dusky scar at the seat of one of the vaccination punctures; but it was not like a normal vaccine cicatrix, and the synechiæ observed in the left eye proved that the organ had been affected with iritis. The patient stated that a month after vaccination one of the punctures inflamed, and became a hard edged ulcer, which lasted three months. In this case the vaccinifer was stated to be a healthy child at the time when the lymph was taken from it, but subsequently it had very unwholesome sores about the anus, for which it was under treatment for three months. Several persons had been vaccinated with lymph furnished by this child, but with two exceptions it was not ascertained whether or not syphilis followed the operation. In the case of the exceptions no ill results were observed.

Mr. Hutchinson considers that these cases and others to which we have referred in a former report prove that syphilis may be propagated by the vaccinator. He asks what we are to infer from the fact that of twelve persons vaccinated from the lymph of a syphilitic child, only one became tainted; and he states that we must either believe that the virus of syphilis is not contained in the lymph at all, or that it is not equally diffused throughout it. It would appear that we may vaccinate from a tainted vaccinifer without conveying the infectious matter of syphilis; and on the other hand it is possible to propagate syphilis with or without vaccinia in the operation of vaccination. Accidentally on many occasions lymph has been taken from tainted children and employed without any bad results. It is therefore highly probable that the virus of syphilis is not contained in the vaccine lymph, but that it is derived from, or at least is associated with, some cell elements of the blood, which need not necessarily be palpably red.

Mr. Hutchinson thinks that with regard to prevention it would be desirable to disseminate as widely as possible amongst medical men a knowledge of the fact that syphilis may be conveyed in the matter used in vaccinating. It would be proper to avoid vaccinating from children whose parents are unknown to the operator. Lymph should not be taken from first-born children, as it would be desirable to wait until the development of one healthy child afforded *primâ facie* evidence that the next one would be free from constitutional taint. Lastly, it is most important to avoid blood-stained lymph, and recent exudations from the walls of the vesicle.

The cases described by Mr. Hutchinson are very strong proofs in favour of his statement that syphilis may be propagated by the operation of vaccination. Such cases must, however, be of the most extreme degree of rarity; nor do they prove that syphilitic poison can exist in the vaccine lymph itself. Even when a tainted child is the vaccinifer only an extraordinary combination of unfavourable conditions can cause it to be a means of communicating syphilis. In one of Mr. Hutchinson's cases twelve persons were vaccinated from the tainted child, and only one—the last—was affected with syphilis. The poison in the blood of the child must have been in a high state of activity, and a portion of it or of its serum mixed with the vaccine lymph, was no doubt the cause of the mischief. It must, however, be a rare case where the blood of a person suffering from any form of syphilis is so highly charged with the poison of that disease that a minute particle of it is capable

of conveying infection. When we consider that there are nearly three-quarters of a million of children vaccinated annually in these countries, it is evident that if cases of vaccino-syphilis were other than barely possible, we should hear of very many such cases as those described by Mr. Hutchinson.

In an official Report^a presented to the Préfet of the Department of Tarn by the Director of Vaccination, Dr. Paul Lalagade, we find the following positive statement relative to the non-existence of vaccino-syphilis:—"Aujourd'hui comme autre fois, et plus autorisé par une plus longue expérience, j'ai l'intime conviction la plus absolue, que le vaccin humain, recueilli et inoculé avec prudence, c'est-à-dire seul, ne transmet et ne peut transmettre aucune maladie en dehors de la vaccine."

Dr. Lalagade states that during his experience, extending over a period of thirty-one years, and relating to 30,000 vaccinations and re-vaccinations, he has never known an instance in which any constitutional malady or syphilitic affection was transmitted by vaccination. At the same time he urges the necessity of caution in the selection of vaccinifers. The child should be more than three months old, healthy, and of healthy parentage.

SMALL-POX AND VACCINATION.

A very interesting paper on the influence of vaccination, age, sex, and occupation, on small-pox, read at a meeting of the Epidemiological Society, 10th April, 1872, by Dr. Robert Grieve, has been since published^b in a pamphlet form. Dr. Grieve is the Medical Superintendent of Hampstead Hospital, and his experience relative to the subjects treated upon in his pamphlet entitles him to write *ex cathedra*.

During the recent epidemic of small-pox in London, 6,221 patients were treated in the Hampstead Hospital. Of these 1,248 were without marks of vaccination, and amongst them the mortality was 51·12 per cent. The deaths amongst the 4,973 vaccinated patients amounted to 567, or at the rate of 11·4 per cent. The percentage of deaths amongst both the vaccinated and unvaccinated taken together was 19·36, a higher ratio than has been observed during recent epidemics.

The mortality amongst the patients who had but one vac

^a La Vaccine et La Petite Vérole, dans le Department du Tarn en 1870 et Albi, 1872.

^b London: J. and A. Chw

street.

scar was 17·39 per cent.; amongst those the deaths were 12·17 per cent. Of the patients showing three scars, 10·15 per cent. succumbed; and the deaths were only 8·38 per cent. amongst those who had four marks. Lastly, the mortality sank so low as 6·43 in the case of those who exhibited five or more cicatrices. Dr. Grieve concludes from these statistics that to obtain even a fair average amount of protection it is necessary to cut in three places, and that it is advantageous even to exceed that number. The patients observed in relation to the number of marks amounted to 3,555.

Out of 6,221 cases of small-pox there were only three in which there was any satisfactory evidence of re-vaccination. The nurses and servants in the hospital were re-vaccinated, and none of them contracted small-pox, though much exposed to its contagion. Dr. Grieve believes that re-vaccination is a sure protection against small-pox, but to be efficacious it must be performed after the age of fifteen years. Cases of variola subsequent to re-vaccination are merely the exceptions that prove the rule; they are more uncommon than second small-pox, and differ also in this way, that whereas the latter are frequently severe and sometimes fatal, the former are very mild indeed.

With respect to the influence of age on small-pox, the extremes of life are unfavourable, according to Dr. Grieve's experience, which is in harmony with that of other observers. Amongst those vaccinated the period of minimum mortality is between the 10th and 20th year of age; at that period the mortality is 35·03 per cent. Under five years the percentage of deaths is so high as 70·87. Unvaccinated infants under one year almost invariably die. Amongst the vaccinated the mortality is also least between the ages of 10 and 20 years, but after 20 there is a greater relative increase in the mortality as compared with the unvaccinated, which Dr. Grieve believes shows that the protective power of vaccination diminishes by the lapse of time.

With respect to sex, although there are more females than males in England, there were 3,377 males admitted into the hospital, and only 2,844 females. The mortality was 18·49 per cent. amongst the females, and 20·13 per cent. amongst the males; but the difference in favour of the females was the result of the greater resistance to the disease in the case of the adults, not of the children. That males succumb more readily to small-pox than females, Dr. Grieve believes may be due chiefly to the former being

more intemperate. "Nothing unfits one to cope with small-pox, and other diseases as well, more than previous dissipation. The likelihood of an imperfectly vaccinated hard drinker recovering from an attack of this disease is, I believe, small."

The influence of occupation in inducing the disease, and increasing the mortality from it, is shown in the following table. The greatest mortality occurred amongst those persons whose occupations conduced most to intemperance, or which exposes them continuously to a high temperature.

Influence of Occupations on Small-pox.

Occupations of Males over 20	Vaccinated			Unvaccinated	
Trades	Admitted	Died	Percentage	Admitted	Died
Blacksmiths	10	5	50	-	-
Cabmen	12	6	50	2	1
Painters	20	6	30	2	1
Grooms	41	9	21.25	1	-
Barmen, &c.	25	5	20	3	1
Butchers	21	4	19.04	-	-
Labourers	132	12	18.93	12	7
Bricklayers, &c.	32	6	18.75	2	1
Drapers and Hosiers, &c.	11	2	18.18	2	1
Jewellers	12	2	16.6	2	1
General percentage of mortality in Vaccinated Males over 20 = 16.46.					
Shoemakers	20	3	15	3	-
Tinmen	14	2	14.29	3	1
Carmen	21	3	14.28	5	4
Tailors	15	2	13.3	3	2
Clerks, &c.	23	3	13.04	1	1
Carpenters, &c.	9	1	11.1	7	2
Gasfitters, &c.	9	1	11.1	1	-
Costermongers, &c.	27	3	11.1	1	1
Porters	67	2	10.44	7	2
Coachmen	11	1	9.09	1	-
Salesmen	14	1	7.14	1	-
Occupation of Females over 20	Vaccinated			Unvaccinated	
Trades	Admitted	Died	Percentage	Admitted	Died
Domestic servants, including	181	39	21.60	18	11
Cooks					
Laundresses					
Sempstresses	27	5	18.51	1	-
	72	10	13.8	6	5
General percentage of mortality in Vaccinated Females over 20 = 13.35.					
Charwomen	20	2	10	4	2

According to Dr. Guttstadt^a vaccination is much neglected in Berlin, and during the epidemic of small-pox in 1871-72, 6,478 perished from the disease. He estimates that 14 per cent. of the unvaccinated inhabitants contracted small-pox, whilst only 2 per cent. of those vaccinated caught the disease. The mortality was 42 per cent. amongst the unvaccinated, 16 per cent. in the case of the vaccinated, and 15 per cent. amongst the re-vaccinated. There was, however, reason to believe that re-vaccination was in many instances not successfully performed.

Dr. Matthew Taylor has published^b the results of some experiments which lead him to believe that re-vaccination or vaccination after small pox rash has appeared, is useless, if not injurious. The experience of Dr. Grieve points to a similar conclusion.

Dr. Fleischmann, in an ably written paper,^c contends that small-pox and varicella are distinct diseases. He maintains that vaccination exerts no influence on the development of varicella; nor does the latter exercise any protective influence in relation to small-pox. Varicella is contagious, but no one gets small-pox from a person affected with varicella, even when inoculation with matter from the pustules of varicella is employed.

"DEATH IN THE POT."

The Anti-adulteration Act, which was passed by the Legislature on the 10th of August, 1872, is beginning to bear some fruit. Public food analysts have been appointed in a large number of the counties and towns of Great Britain and Ireland, and several persons have been prosecuted for selling adulterated articles of food and drink. In Dublin, in the year 1872, 67 persons were convicted for selling adulterated food, and the fines and costs imposed amounted to £308 10s. Out of 500 specimens of food, drink, medicine, tobacco, and snuff, 164 were adulterated. Milk was sophisticated with water; butter with fats and excessive amounts of water; flour with alum and rice; rice flour, with white clay (*terra alba*); tea, with tea leaves which had been exhausted of their soluble matters and re-dried, catechu, and various foreign leaves; and coffee with chicory and burnt sugar. The confections collected for analysis were pure, owing to the fact that many sweetmeat manufacturers had been fined in 1870 and 1871 for selling

^a Transactions of Berlin Medical Society, Dec., 1872.

^b The Lancet, February 1, 1873.

^c Published in the Alleg. Wiener Med. Zeitung. No. 7. 1873.

articles adulterated with plaster of Paris and terra alba, and coloured with poisonous pigments. Several samples of adulterated wines and of spurious foreign brandy were analysed; but the whiskeys examined, though in many cases very weak and almost new, were free from adulteration.

In London and other places tea grossly adulterated, butter containing flour and various foreign fats, bread contaminated with alum, vinegar including a sensible amount of muriatic acid, and mustard made up largely of flour and turmeric, have been reported upon; as were also many other sophisticated articles of food.

The prevalence of food and drink adulteration in these and other countries must injuriously affect the public health. It is, however, the children of the poor who are the greatest sufferers, for the milk which forms so large a portion of their nutriment is systematically adulterated in every place. Much injury is also done to the public health by the consumption of the flesh of animals which have been killed or have died whilst suffering from disease, and by the use of putrid, musty, rancid, and otherwise inferior food—animal and vegetable. It is a matter beyond doubt that in most parts of the United Kingdom the flesh of diseased animals is sold without any interference on the part of the local authorities. It is only in a few of the larger towns that there is any systematic inspection of the food of the people. In Dublin last year 333,016 pounds weight of diseased or putrid meat was confiscated, and three persons were imprisoned, and twenty-five fined in the aggregate amount of £91 12s. for trafficking in the flesh of diseased animals.

In a former Report we referred to numerous cases of disease and death produced by the use of diseased meat, and since then fresh proofs of the unwholesome character of that kind of food have been published. Dr. M. Pétry, in a communication* to the Belgian Royal Academy of Medicine, refers to the frequent production of carbuncular disease in man from the use of the flesh of diseased animals. He gives us the history of an outbreak of malignant pustules amongst the inhabitants of the village of Condroz, which was clearly traced to the consumption as food of the flesh of an ox affected with charbon.

In No. 7, Vol. 6, of the *Bulletin de l'Académie Royale de Médecine de Belgique*, M. Kuborn, in a lengthy article relating to the injury inflicted on the public health by the consumption of diseased

* *Considerations Pratiques sur le Typhus Bovin et Charbonneux. Bulletin de l'Académie Royale de Médecine de Belgique. Année, 1872. Tome vi. No. 9.*

meat, proposes that the carcasses of all animals affected with contagious diseases should be burned. He describes a process of incrimation which would not give rise to a serious nuisance, and he considers that his plan, if perfectly carried out by the authorities, would eventually destroy the parasitic and contagious diseases of animals used as food.

Many cases of poisoning by shell-fish are on record. In 1827 nearly thirty persons were poisoned by mussels at Leith. In the *Lancet* for February 15, 1873, a case of poisoning by these molluscs is reported. The schoolmaster of the Union Workhouse, Falmouth, took fifteen of the boy inmates of that institution for a walk on Mainford beach. It would appear that they picked up and ate some mussels, which were abundant on the rocks. One of the boys speedily fell down in a fit, and soon after three other boys became similarly affected. Three of the boys died within an hour, and the fourth recovered. Many kinds of shell-fish and fish occasionally assume poisonous properties, from causes which at present are unknown. In one instance, however, Bouchardat proved that the poisonous effects produced by mussels taken from a particular place was due to the presence of copper in the shell-fish.

A case of poisoning by rancid butter came under our observation in Dublin last April. A tradesman and his family became unwell after partaking of breakfast, consisting of tea, cold meat, bread and butter. The father complained at the meal of the quality of the butter, and with difficulty forced himself to swallow it; shortly afterwards his stomach became sick, and, to use his own words, he felt the rancid butter coming up his throat. Two of his daughters became very unwell almost immediately after breakfast; they suffered severely for some time from choleraic diarrhœa. They were admitted into the Mater Misericordiæ Hospital, where the younger girl, Julia Cummins, aged eight years, expired in the evening. The contents of her stomach, the matter ejected from the father's stomach, and portions of the unconsumed food from the morning meal were submitted to us for analysis, but no poison was detected therein. The meat, of which only a small portion had been eaten, presented all the characteristics of the flesh of a healthy animal. The butter, of which only a minute quantity came into our possession, was rancid in the most extreme degree—indeed its odour alone was almost sufficient to make a delicate person ill. We felt no hesitation in coming to the conclusion that the butter had poisoned the family;

and the verdict of the coroner's jury, who inquired into the case of Julia Cummins's death, was in harmony with that belief.

A remarkable instance of the propagation of typhoid fever by means of milk has been recorded by Dr. Ballard, of Islington. Another case of this kind is described in the *Medical Times* for March 15, 1873, by Dr. Russell, Medical Officer of Health for the City of Glasgow. In the suburb of Parkhead, enteric fever, previously a rare disease in the locality, became prevalent. The water used in the district being good, the sewerage arrangements satisfactory, and the people "comfortable, cleanly, and well living," an outbreak of enteric fever surprised the sanitary authorities. Dr. Russell made inquiries, and found that the earliest case of typhoid fever occurred in the house of a dairyman: out of seventy-three families (in five streets only) supplied with milk by this man, twenty-two had fever; whilst out of 146 supplied with milk by other vendors, only two contracted typhoid fever.

Dr. Russell did not get the milk vended by this man examined to ascertain if it were adulterated with water; but supposing the milk which conveyed the contagion had been mixed with water, it would have been the pure liquid from Loch Katrine, which certainly could not have contained fever germs. In Dr. Ballard's case there was a very strong presumption that the milk was adulterated with contaminated water. In the Glasgow case the milk must have directly absorbed the virus of the disease, probably from the hands and clothes of those who manipulated the liquid. On this point Dr. Russell says:—

"The period of incubation of enteric—i.e., the interval which may elapse between the introduction of the poison into the system and its activity—may extend to three weeks. This carries us back to the end of December, when Mr. X.'s family had enteric fever, when the germs of the disease on one or more occasions got into his milk, and were distributed to those unfortunate persons who were one by one attacked during the months of January.

"I regard this as an extreme illustration of what must frequently happen where the sale of articles of food is conducted in close connexion with families, with all their attendant ailments. Milk is, from its composition, a peculiarly favourable medium for the propagation of the germs of disease, and particularly of enteric fever. It is seldom that this fever is diffused by milk in circumstances which permit us to trace the disease home to the milk so clearly as in the Parkhead case; and it is very likely that many apparently inexplicable outbreaks of enteric fever

in families are caused by milk, or even solid food, contaminated in the retail shops, especially among the poor. It is a very common practice in all parts of the city for parties to live and rear families in rooms behind shops, through which often the sole access lies, and in which groceries, milk, provisions of all kinds, sweetmeats, fruit, &c., are sold. These shops are 'served' by one or both parents, or some grown-up child, and when infectious disease enters such a family it cannot fail to be the source of quite peculiar risk to the public."

Dr. M. K. Robinson, medical officer of health for Leeds, in his report on the sanitary condition of the borough for 1872, describes an outbreak of enteric fever similar to the Parkhead and Islington cases. More than one hundred customers of a milk vendor were affected with this disease immediately after it had appeared in the dairyman's house. Dr. Ballard, medical inspector of the Local Government Board, investigated the case, and came to the conclusion that the disease had been spread through the medium of milk.

INFLUENCE OF OCCUPATION ON THE DEVELOPMENT OF PHTHISIS PULMONALIS.

At a meeting of the Royal Medical and Chirurgical Society, held on the 14th January, 1873, a paper by Dr. Alexander Rattray was read, entitled "An Analysis of Ship Air and its effects." The author states that in H.M. frigate *Bristol* the sleeping space is little more than from 100 to 120 cubic feet per man (amongst the crew), and at their meals the space is only 60 cubic feet. The air under such circumstances can hardly be other than extremely impure. According to Dr. Rattray it often contains more than 1·8 per cent. of carbonic acid, a proportion which is only to be met with in deep mines. It is almost incredible that sailors could habitually sleep in an atmosphere containing from 0·4 to 1·8 per cent. of carbonic acid and to retain good health and bodily vigour. The experiments of Angus Smith and others show that at a much less impure state of the air the vital powers subside to an alarmingly low point; and when the air contains 4 per cent. of carbonic acid, candles actually go out. According to Dr. Rattray the impurity of the air in ships chiefly leads to complaints of the respiratory and circulating systems, *e.g.*, catarrh, bronchitis, and phthisis. In the table which we give below the mortality from phthisis amongst sailors is high.

The Report of the Board of Health of the Health Department of the City of New York, for 1872, contains a most interesting

statistical table, showing the mortality by phthisis pulmonalis amongst persons engaged in various pursuits in that city. The figures given refer to a period of three years, and the numbers dealt with are of sufficient magnitude to afford fair average results.

Table showing the influence of various occupations upon Phthisis Pulmonalis in New York City.

Occupation	No. of Persons engaged in (Census 1870)	Average No. of Deaths annually by Consumption in 1869, 1870, and 1871	Deaths by Consumption in each 1,000 persons
Laborers (including Quarrymen, Agricultural Laborers and Porters) -	28,700	329.3	11.5
Coopers -	1,606	13.6	8.5
Machinists (including Blacksmiths and Metal-workers)	11,178	92.3	8.2
Lawyers -	1,283	10.0	7.8
Seamen and Watermen (including Sailors and Steam-boatmen) -	4,463	34.6	7.7
Boot and Shoemakers -	6,960	54.0	7.7
Barbers (including Hairdressers) -	2,549	18.3	7.2
Carmen (including Coachmen and Teamsters) -	9,813	69.3	7.1
Printers -	5,134	36.0	7.0
Painters (including Varnishers) -	5,824	40.0	6.9
Masons and Stonecutters -	6,586	44.6	6.8
Carpenters (including Cabinetmakers, Upholsterers, and Joiners) -	15,498	103.0	6.6
Dressmakers (including Milliners, Mantuamakers, Tailoresses and Seamstresses) -	18,614	120.0	6.4
Teachers (Female) -	2,173	13.0	6.0
Tailors -	9,697	55.6	5.7
Bakers -	3,855	21.3	5.5
Bookbinders -	2,276	12.0	5.3
Cigarmakers (including Tobacco-workers) -	5,550	29.0	5.2
Domestic Servants -	49,440	253.0	5.1
Butchers -	4,870	22.3	4.6
Clerks (including Salesmen and Accountants in stores, banking, brokerage, insurance and manufacturing establishments, and Civil Employés of Government)	35,432	163.0	4.6
Hatters (including Hat and Capmakers) -	1,744	6.6	3.8
Physicians and Surgeons -	1,741	6.6	3.8
Peddlers (including Hucksters and Commercial Travelers) -	4,744	18.0	3.8
Merchants (including Traders and Dealers) -	23,872	75.3	3.1
Stablemen (including Livery Stablekeepers and Hostlers) -	1,278	4.0	3.0
Teachers (Male) -	1,338	3.0	2.2

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Wednesday, 7th May, 1873.

DR. DARBY in the Chair.

Pulmonary Hæmorrhage a cause of Consumption—(Discussion resumed from p. 460).

DR. M'SWINEY—Many important topics for consideration are suggested in Dr. Finny's able paper, but the main issue raised in it is—as it seems to me—this one, namely, “Is pulmonary hæmorrhage ever a cause of phthisis?” Two modes of treating the subject thus proposed present themselves to us; the discussion of this question may be undertaken in a strictly scientific spirit, and much light may be expected to be shed upon what is obscure in it, by reasonings derived from abstract or theoretical sources, and from the results of experiments upon animals. Its consideration may also be approached from the merely empirical and practical point of view, and from knowledge supplied by clinical experience. I am not competent to use the former method with sufficient skill, and—highly as I esteem the labours of the cultivators of science—I am tempted, when I read the discussion at the London Pathological Society, to exclaim, with the great clinical physician Trousseau, “Gentlemen, let us have a little more art and a little less science.” For the reason just stated, then, the few remarks I will make shall be altogether in the direction of the empirical mode of considering the investigation upon which we are engaged. Referring now to the subject of pulmonary hæmorrhage, I would first remark, as necessary to my argument, that expectoration of blood is liable to be present at any time—and in almost any quantity—during the course of phthisis; but clinical experience would appear to show that three different periods may be noted in particular for the occurrence of hæmoptysis in the progress of a case of consumption. In the very early stage of the disease there may be one, or several attacks of profuse hæmorrhage; there may, also, be occasional bloody expectoration in the progress of the disease; and finally, sometimes—but rarely—the

disease may terminate fatally by rupture of a large vessel in, or adjoining, an excavation of the lung. But clinical records also abundantly show that hæmoptysis does very often occur without being in any way connected with or followed by consumption. I think this a very important circumstance, which has not received all the attention it merits from the advocates of the doctrine, "Hæmoptysis a cause of Phthisis." I may be permitted to cite a few examples. In what Sir Thomas Watson calls "habitual hæmorrhages," the blood sometimes proceeds from the bronchi. In mitral obstructions and other cardiac lesions; in disease of the kidney, or of the liver; in purpura hæmorrhagica, and in asthma; in scorbutus; after great muscular exertions, particularly where the atmospheric pressure is diminished, as in Alpine ascents,—in all these, and in other cases, hæmoptysis has been observed sometimes to occur in persons free from lung disease at the time, and in whom consumption did not subsequently set in. In suppression of the menses, profuse pulmonary hæmorrhage occasionally—no doubt rarely—takes place, and may even be continued regularly at the monthly periods. Hæmoptysis is also often present, in great quantity, in injuries and wounds of the chest. Speaking of this symptom, Salter, ("On Asthma," p. 89) says, "In one case that came under my observation, this hæmorrhage occurred for years at almost every attack. And yet no permanent injury was sustained by the lungs, and the ultimate recovery of the patient was complete. . . . And, although the quantity is generally small—a teaspoonful or so—I have known it to amount to a profuse hæmorrhage, from half a pint to a pint." But although this be admittedly the case, we, nevertheless, also know that copious bleeding from the lungs frequently happens in persons who present no sign of deranged health, or symptom of consumption at the time, and in whom, however, the disease plainly manifests itself soon after the first hæmorrhage, and occasionally—as in Dr. Finny's cases—runs a rapidly fatal course. The question then, here is—In those cases of hæmoptysis which are thus followed by consumption, must the occurrences be looked upon in the relation of cause and effect? Undoubtedly the opinion widely prevails at the present time, and is expressed in the phrase "*phthisis ab hæmoptoe*," formerly used by Morton, that pulmonary hæmorrhage is one of the most potent of the exciting causes of consumption. And it is acknowledged that in numbers of recorded cases there were no symptoms complained of, or any objective appearances presented, tending to raise the least suspicion that the patients were the subjects of the consumptive diathesis before the outpour of blood. Here the conclusion would appear reasonable that in these instances the hæmoptysis marked the starting point and was the cause of the disease. This is Dr. Finny's view, and he is supported in it by many high authorities. I venture to think, however, that the position here taken up is not an impregnable one, and that its occupants may be dislodged. In

the first place, if we are to adopt this opinion, how can we reconcile it with the undisputed fact, that blood effused into the air-cells—and often in large quantity, and frequently repeated—does not, in many instances, as we have already seen, prove the source of any serious and permanent irritation in the pulmonary organs? It is true that Andral's experience was, that of the persons whom he had known to die of phthisis, two in six had a hæmoptysis which preceded the other symptoms of the disease. And Fournet also adopted the notion that hæmoptysis is sometimes an exciting cause of phthisis. But Louis, who perhaps over-estimated the importance of hæmoptysis, in his treatise on phthisis, after the most exhausted consideration of the symptoms of the disease says ("Researches on Phthisis," p. 505): "It is impossible, then, in the existing state of things, to regard hæmoptysis—either of considerable or of trifling amount—as a cause of tubercles." I also claim Dr. Williams as being opposed to the doctrine under discussion. He says—as quoted by Dr. Finny—that hæmorrhage from the lungs in these cases "is indicative of a fragile state of the vessels, closely connected with, and arising out of consumptive disease of these organs." If it be granted that the bleeding is due to "a fragile state of the vessels . . . arising out of consumptive disease" of the lungs, then everything is conceded. As it appears to me the premises in the argument are assumed by the advocates of "Pulmonary Hæmorrhage a cause of Phthisis." Theoretical speculations would seem to have conducted them to a conclusion which it only wants clinical and pathological facts to confirm, but which, I maintain, is opposed to experience derived from both these sources. In addition to what has been previously said, I may here recall to your recollection, as tending to support my argument, a point in practice well known to competent observers, which is, that the crepitation in the lungs which immediately follows hæmoptysis usually disappears in a few days. The crepitation was due, doubtless, to the effused blood. Does not its disappearance denote that all the blood has been absorbed? But, if so, how then can it act as a source of irritation, producing phthisis? Perhaps an explanation of such cases as these under discussion, different from that offered by Dr. Finny, is to be found. I assume that the position is—that hæmoptysis is—in such cases as those narrated by Dr. Finny—the cause, the *Fons et origo*, of the subsequent phthisis; for if this be not the argument there is nothing substantial to dispute about. But, I would ask, may not the prior existence of a constitutional or general morbid condition, causing a receptivity for the phthisical degeneration in the lungs, account for the apparent agency of hæmoptysis in originating the disease? May we not, in fact, believe that the predisposition to the deposition of tubercle has produced the hæmoptysis—not the hæmoptysis the consumption. When hæmoptysis, not followed by consumption, occurs in a patient, we know, generally, to what cause it is due. But when the pulmonary hæmorrhage, not attributed to any of

these other commonly known causes to which I have before referred—occurs, and is followed by consumption, either there are already tubercles (or some allied morbid product) in the lungs, or there are not. But if no tubercle (or allied morbid product) already exists in the lungs, what then, produced the hæmoptysis? The cause can only be, I venture to say, tubercles, or the tubercular or consumptive diathesis already existent in the patient.

But, moreover, may not tubercles be deposited in the pulmonary organs for some time without giving any appreciable indication of their existence there, either by symptoms in the general health of the patient, or by physical signs in the chest? There is nothing inconsistent with experience in supposing that they may—and I am persuaded this is, sometimes, actually the case. In fine, the results of my clinical observations of the symptoms presented by patients affected with consumption, and of my pathological experience, derived from the examination of the lungs of those who have died from the disease, has been to produce in my mind a strong opinion that hæmoptysis, soon followed by consumption, never occurs except in persons in whose lungs either tubercles (or allied morbid products), or the disposition—inherited or acquired—to the deposition of tubercle already exist.

This is the most important point raised in Dr. Finny's paper, but as he has referred to the treatment of hæmoptysis, I beg leave to say a few words on that subject. Before, however, considering the treatment proper to apply in pulmonary hæmorrhage in consumption, a preliminary question, requiring to be answered, arises, whether—namely—it is desirable, in every case, to use means to arrest the bleeding at once. If we grant Dr. Finny's premises, the conclusion follows of necessity, that it is always most essential to put a stop to the hæmorrhage. Yet it certainly would appear, as the result of experience, that this bleeding sometimes relieves the urgent symptoms very much—that the patient gets better after it; and the fatal result is postponed for a considerable time. Even recoveries—where great hæmoptysis has occurred—are known to have taken place. And, on the other hand, some of the most rapidly fatal cases of pulmonary consumption met with, have run their entire course without one drop of blood appearing in the expectoration. Are we not justified in saying that, in fact, hæmoptysis, in phthisis, seldom leads to any evil results—that its influence, indeed, on the progress of tuberculosis would seem to be, as a rule—as shown by clinical observation—rather favourable than otherwise. For myself, regarding it as being often curative in its tendency in phthisis, I incline to the opinion that it should not be too promptly suppressed in those cases where it is not alarmingly profuse, and where it may be likely to afford relief to distressing symptoms. But a large number of cases are met with, where our duty is imperative to arrest the bleeding. In these cases I have found perfect quietude of body and of

mind, strict avoidance of excitement in any form, and the use of ice, and iced, unstimulating drinks, far and away the best remedies to rely upon. From drugs I have not been able to obtain all the aid and assistance I would desire. Ergot—from its special property of stimulating the unstriated, involuntary muscular fibre, and thus of producing contraction of the muscular coats of the arteries—is probably the best hæmostatic. Dr. Finny has found it efficacious, and I can confirm, from my own observation, his experience of its utility. I have never been quite satisfied that *opiates* were useful—or even allowable—in bleeding from the lungs; but I have often felt convinced that dry cupping over the thorax did much good, and was a valuable means of checking excessive hæmoptysis.

DR. GRIMSHAW said he had seen a considerable amount of phthisis and a large number of cases where hæmoptysis had been a serious symptom. He entirely agreed with Dr. Finny, that hæmoptysis might be a cause of phthisis, but in saying so he must throw the question of tubercle out of consideration altogether. Whatever be the nature of tubercle, there were many kinds of phthisis which had no tubercle connected with them. As to whether hæmorrhage was a cause of phthisis or not depended altogether on the amount of damage this effusion of blood may do in the lung of a person who otherwise was liable to phthisis. It was a fallacy to say of a patient who had had hæmorrhage, and whose lungs had undergone phthysical disease, that his disease was not caused by hæmorrhage, but by a phthysical constitution, or by living in a phthysical locality. He held that a person living in a phthysical locality would not get the disease until some exciting cause happened to him, any more than a person who had not some predisposition would suffer from a knock on the knee or a broken bone. Some persons who received such injuries would rapidly get well, while others, who had a scrofulous diathesis, would suffer severely. He looked on the effusion of the blood into the lung as in the nature of an accident, and when that effusion occurred from any cause to a person of a phthysical tendency it would establish those destructive changes in the lung which were known by the term phthisis. He must protest against the word tubercle being introduced into the present discussion. Dr. M'Swiney had alluded to the disappearance of the crepitation caused by effused blood, which he regarded as a sign that the blood had vanished from the lung; but the injurious portion had not vanished; the solid constituents of the blood remained behind, and excited inflammation, in fact, a circumscribed pneumonia, which may follow any of the ordinary courses which pneumonia takes, and gives rise to phthisis and destructive lung changes, according to the constitution of the patient. This was the real way in which hæmoptysis might be regarded as a cause of phthisis. He remembered one well-marked

case, where a person who had a hereditary predisposition to phthisis, but had no marked characters which might be called phthisical in itself, had an attack of hæmoptysis. The cause was evident. He was in a band, and was playing a wind instrument, and was attacked while playing. He came into hospital, and he (Dr. Grimshaw) found a crepitation in a particular portion of the left side of the chest behind. He expectorated blood, and afterwards he had a slight febrile attack that lasted a few days, and then subsided. He got apparently quite well, and left the hospital, but he went out with a dull spot on the back of the left chest about two inches in diameter. While in hospital he lost no flesh, and had a good appetite. In a month after leaving, his cough returned, and expectoration of purulent matter commenced, the left lung was greatly broken up from the base upwards, and the patient died of well-marked phthisis. That was a case in point, and it must not be argued that because a large proportion of those who get hæmoptysis do not get phthisis, therefore hæmorrhage from the lungs was not to be looked on as a cause of that disease. He believed it to be a very efficient cause. It did not follow that it was necessary to control hæmorrhage because it was a cause of phthisis. Once blood was effused in a particular place it should be dislodged from its position as soon as possible, but it did not at all follow that it might not be useful to get rid of more blood, and thus control the inflammation that might arise from the effusion. He had found extraction of blood from other parts diminish the attack where there was not a predisposition to phthisis, and he believed it was a useful mode of treatment. The point before the Society was that hæmoptysis may give rise to a change in the lung which may give rise to phthisis, which must be regarded from the old point of view as a wasting of the body caused by destructive disease of the lung.

The CHAIRMAN said this was a very wide subject, and opened up a large field for discussion. They had now, he might say, two papers before them on the subject, for Dr. M'Swiney's written speech assumed the character of a well-considered and well-written essay—it appeared to him that any case in which hæmorrhage from the lungs occurred in a cachectic individual the case might, with reasonable probability, be suspected as one of general phthisis; but they must all have seen cases of acute hæmorrhage from the lungs which did not terminate in phthisis. The two papers before them discussed the subject from different points of view, and Dr. Grimshaw, in his speech, had laid it down that phthisis was not necessarily connected with hæmorrhage. He (the Chairman) perhaps be permitted to say that the Society that his friend Dr. M'Swiney, while denying that hæmorrhage suggested an anatomico-physiological theory—some

The specific function of the lung is to bring the blood into the closest possible contact with the atmosphere. The trachea and larger bronchi are permanently open air tubes, supported by strong dense elastic tissues; these as they go on diminish in size and thickness, and their coats become "smaller by degrees and beautifully less," and at last merge, as it were, into the coats of the blood vessels, themselves wonderfully attenuated, until it may almost be said they ultimately branch in the open air. In such a refined structure it is easy to comprehend how readily hæmorrhage may take place, but difficult to understand why it does not more frequently occur.

DR. HENRY KENNEDY on rising said, it is now about two years since I had the honour of reading in this place a paper on the views of Niemeyer regarding phthisis. I did not hesitate then to criticise those views as not agreeing with well-ascertained facts, and as not taking a comprehensive view of the subject. On the general question I shall not now say anything, but pass it by, merely making a single remark. If nutrition be carried on by inflammatory action, then the German views may be correct. If the function which changes the food we eat into the various tissues of our bodies requires inflammation for its due performance, then the deposition of tubercle may be due to the same process. But as I believe that none would support this idea, so I think the latter falls to the ground. It is obvious, Sir, that the point so well brought before us by Dr. Finny is but a part of the larger question, and to this point I shall now confine myself. I would remind the meeting, in the first place, of the more common forms of pulmonary hæmorrhage which we are in the habit of meeting. This pulmonary apoplexy is a disease, or a symptom of a disease, of by no means unfrequent occurrence. I have treated a good many such cases, and have examined several after death. But I neither observed during life, nor found subsequently, any trace of tubercle; and yet, if the views of the late German physician be correct, these are cases which would seem to invite, as it were, to such a result. And similar remarks may be made of the disease known as cirrhosis. In both these diseases, in fact, extensive and repeated hæmorrhages occur and recur, and yet, it has not been my lot to meet any case amongst them like what Dr. Finny has detailed. Again, there are bleedings into the lung in connexion with aneurism, the weeping aneurism as it is called. These, I admit, are very rare cases, but they do occur. But there are cases of much more frequent occurrence; I mean those which are met with in females in connexion with deranged menstrual function, and cases in which, for want of a better term, I call constitutional hæmorrhage, seen in both sexes, and not in any particular form of phthisis. I confess it seems difficult to reconcile the terms of hæmorrhage with the cases we have heard detailed,

for, I repeat, it has not been my lot to meet in any of them results similar to those Dr. Finny has described. These latter then, I take it, must be in the very highest degree exceptional cases, and possibly, indeed, this may at once be conceded by the other side. But I will be asked here, have you not seen cases like them? To which I answer at once that I have; but they were, or, at least, seemed to me to be in connexion with phthisis, or the strumous diathesis. And this leads me to speak of hæmorrhage in its relation with phthisis. It seems to me that the weight has not been given to the great varieties of ways in which bleeding shows itself in these cases. Sometimes it seems to usher in the disease, as in the cases given by Dr. Finny; again, it appears after tubercles have been known to have lasted for some time, or bleeding may never have occurred till the last moment of existence. That it may be entirely absent too is, of course, well known. Now, in the first two—that is where bleeding occurs at the very onset, or in the middle stage of the disease—it frequently occurs without causing any of the symptoms which appeared in Dr. Finny's cases. So far from bringing the disease, it actually seems to lengthen it, and the patients frequently express marked relief from the bleeding, and will live the average period, which may be assumed to be two years. As in the cases detailed the longest lived only five months, I need scarcely remark what a marked contrast they afford to the more ordinary course of the disease. And when I consider the symptoms they presented, and especially the much greater amount of disease in the base of the lungs than elsewhere, the contrast becomes the more striking, and the interpretation which I would venture to put on them must be very different indeed from what they have received. It would certainly have been very desirable that the cases had been examined after death. That we may assume was not possible. Fortunately, however, since the reading of Dr. Finny's paper a case exactly in point has been recorded by Dr. Yeo in the *Irish Hospital Gazette* for April 15th. It is entitled "A Case of Acute Tuberculosis," and began by severe hæmorrhage, and ran its course in eleven days. Certainly, I must say the German views are fast leading on to conclusions which, to say the least of them, are very difficult of acceptance, and, for myself, cannot be accepted at all; and were there time I think an analysis of this special case would show that it was anything but favourable to the opinions which are now so generally received. It is, however, enough for my present purpose to state that the *post-mortem* showed there had been old standing disease. In other words, the hæmorrhage was not the starting point of the attack. And so I take it to have been in Dr. Finny's cases. I believe it to be next to impossible that bleeding will occur in a healthy state of the system, and in the next place, if it do occur, that it will not cause the deposition of tubercle. I believe that unless the diathesis be present no tubercles will be formed. It has been argued that when blood

has been effused elsewhere, as well as into the lung, it may cause inflammation and fever of a bad type. This I admit, but consider it a very different matter indeed from the subject in hand. I take this to be the true explanation of the three cases detailed, in each of which I believe there originally existed the strumous diathesis, and also at the time the bleeding occurred an unhealthy state of the system, leading on to the bad forms of pneumonia detailed by Dr. Finny. It is worthy of remark that pneumonia *per se* will often run an analogous course to this; that is where the constitution is in a bad state at the time when the pneumonia supervenes; and in cases of undoubted phthisis, where cavities existed, I have met some instances exactly like those detailed. This explanation, I repeat, appears to me the most worthy of acceptance. It squares best with all the experience I have had of the subject, and it does not contradict the many points in the natural history of phthisis which I take to be absolutely established.

DR. HAYDEN said the question before the Society was one of such great importance that it ought not to be allowed to pass from under their notice without eliciting from every practising physician present a free and candid experience of his views upon it. There could be no doubt that theories and doctrines in medical science were liable to extreme fluctuation. A few years ago Laennec's doctrine was predominant in all the schools of Europe, that doctrine which implied that pulmonary disorganization was invariably tuberculous, and that tubercular deposition was always primary and constitutional. That doctrine was undoubtedly too exclusive, but there could be as little doubt that the doctrine which had in modern times displaced it was too exclusive also, namely, that which implied that pulmonary consumption is generally of inflammatory origin, and but very rarely tuberculous and primary. He supposed that after a period equal to that which had elapsed since Laennec's time the successors of the present generation of physicians might find no less valid reasons to question the existing doctrine than those now urged to discredit the doctrine of Laennec. Dr. Finny's paper, eminently suggestive, raised two questions. First, "was latent tubercle of the lungs a cause of pulmonary hæmorrhage?" and second, "may pulmonary hæmorrhage, when it has occurred, be the cause of pulmonary consumption?" He agreed with Dr. Grimshaw, that in considering this question it would be much better to leave out of view altogether the question of the nature of tubercle. There could be no doubt at all that there might be pulmonary consumption in the absence of tubercle, that is, there might be a form of disorganization of the lungs previously inflammatory. In answer to the two questions he had put as implied in Dr. Finny's paper, he would venture to answer "yes." He believed that latent tubercle might be a cause of pulmonary hæmorrhage, and that

pulmonary hæmorrhage might be a cause of pulmonary destruction or disorganization. Dr. Finny had implied, and Dr. M'Swiney had admitted that primary hæmorrhage from the lungs might be associated with a strumous diathesis. Endorsing this statement in its entirety he would ask Dr. Finny what "strumous diathesis" meant? He thought any practising physician could answer the question promptly that strumous diathesis implied an extreme vulnerability of the constitution of the patient, a delicacy of texture and great susceptibility to injurious impressions from external agents. If they analysed the structures they would find they were characterized by extreme delicacy, and this, he had no doubt, extended to the pulmonary vascular system.

If they assumed an extreme tenacity of the pulmonary capillaries it was not difficult to conceive that a tubercular deposition might exist in the lungs so disseminated as not to give rise to signs or symptoms. He had seen cases in which there were symptoms of deposition in the lungs without physical signs, and cases where there were signs without symptoms. For example, he had seen at least two cases where the physical signs were well pronounced and no cough whatever existed. In a case such as that, when there was a latent deposition of tubercle, and at the same time extreme tenacity of the vascular walls, was it a forced assumption that the tubercle might become a centre of irritation—that it might give rise to an afflux of blood to the lung, and thus that it might be the determining cause of a capillary hæmorrhage, which, under the circumstances, might be copious? He believed that pulmonary hæmorrhage might be secondary to latent tubercular deposition in the lungs. As to the second point, whether extravasation of blood in the lungs might be a cause of pulmonary consumption, he believed it might be so. He had seen cases where there was no reason to suspect primary tuberculosis of the lungs, and where, under some accidental circumstance, such as a blow on the chest, hæmorrhage had taken place, and resulted in pulmonary consumption; but he believed that in all these cases there was a predisposition to consumption. He believed further that there must be copious extravasation of blood. Louis remarked that a trivial extravasation of blood would not give rise to those consequences. Every one of them must have witnessed examples of spitting of blood, or even expectoration of blood by teacupfuls, and yet recovery took place with only slight febrile reaction; but how rarely did they see a copious hæmorrhage followed by such a favourable result, where a tubercular diathesis existed. On the other hand, they found copious hæmorrhages without any such consequences where there was no family history of and no predisposition to tuberculous consumption. He thought his friend Dr. Grimshaw, was extreme in his view when he stated that the expulsion of localized effusion of blood was not the rule. He (Dr. Hayden) thought it was the rule, and that the consolidation of extravasated blood in the lung was the exception.

In those cases in which recovery occurred, there was a complete clearing of the lung, the process often extending over several days, and the blood towards the end being dark and partially coagulated, thus showing that it had for some time lain in the vesicular structure of the lung. He repeated that he thought two conditions were necessary in order that pulmonary consumption shall follow pulmonary hæmorrhage, namely, the occurrence of a copious hæmorrhage, and the existence of the "strumous diathesis." As to treatment, ice was the safest and perhaps the most efficacious hæmostatic where hæmoptysis is *active*, that is, associated with quick and bounding pulse, and hot skin. But of drugs, he had the greatest faith in ergot of rye, given in doses of 15 m. of the liquid extract every half hour or every hour, according to the urgency of the hæmorrhage.

DR. GRIMSHAW did not mean to imply that as a rule the blood effused in pulmonary hæmorrhage was not expectorated. He believed as a rule it was, but that in order that it should be a cause of phthisis it should be retained.

DR. GERALD YEO said that he quite agreed with Dr. Kennedy in considering that the small encapsuled cavity, which had existed in the case of acute tuberculosis published in the *Irish Hospital Gazette* could not have been formed in so short a time as eleven days; and he thought that in the notes of the case, he had particularly expressed the opinion that this cavity had existed for some time before the patient had come under observation, and that it was only connected with the disease which caused the death as a probable source of the hæmorrhage. He considered that the case in question rather supported Dr. Finny's views, with which he concurred, though Dr. Kennedy had adduced it as a proof against *phthisis ab hæmoptoe*.

He could not agree with Dr. Kennedy that pulmonary apoplexy was *never* a cause of phthisis; on the contrary, he thought that small apoplexies (hæmorrhagic infarctions), in themselves not sufficient to produce immediate death, frequently caused destruction of the lung tissue, and thus phthisis. But of course in those cases of hæmorrhagic infarctions which are large enough to cause rapid death, there is no time for any such changes to occur, and these are the cases which are most easily recognized by the clinical physician as pulmonary apoplexy.

DR. LITTLE wished to express his general concurrence in the view that Dr. Finny had brought forward. The facts appeared to be simply these, that in many cases, hæmoptysis was the first symptom of an illness that ends in phthisis; and secondly, that cases of phthisis sometimes go on for months, and even for years, very slowly, until a hæmorrhage takes place,

after which the progress of the disease is rapid. If they took those two facts they were driven to the conclusion that probably hæmorrhage must play an important part in the destructive changes that constitute phthisis. He did not think the question of the existence of tubercle in cases of phthisis was part of the issue in the present discussion. Repeated examinations in the dead house, however, had convinced him that pulmonary consolidations and excavations (the essential anatomical changes in phthisis) were frequently found without there being present a single one of the granular bodies to which the term tubercle was usually applied. The question Dr. Finny had raised appeared to be this—Is hæmorrhage the consequence of pre-existing disease or the cause of subsequent disease? Dr. Finny had adopted the latter view, stating however that there must be two factors at work—one the occurrence of hæmorrhage, which may be determined by accidental circumstances, and the other the pre-existence of a constitutional tendency. If he differed from Dr. Finny it would be in giving greater weight to the latter and less to the former. He had repeatedly seen pulmonary hæmorrhage occur without any bad effect, more particularly in cases of mitral valve disease, in which he did not ever remember having seen symptoms of consolidation and excavation ensue; and it was only in cases in which there existed before hand a tendency to phthisis that he had seen fatal results from pulmonary hæmorrhage. By a strumous or scrofulous tendency he meant a tendency to a low form of inflammatory action, and it was only in those cases that hæmorrhage into the lungs was followed by destructive changes.

MR. FLEMING alluded to cases of severe contusions of the chest, which he had witnessed, where, almost immediately after the accident, pulmonary hæmorrhage had occurred, and where no injury to the walls of the thorax beyond extended ecchymosis could be detected. In some of those cases very large extravasations of blood appeared in the site of the injury, involving the surrounding subcutaneous, and deeper seated areolar tissues. In their earlier stages those extravasations were unaccompanied by any discolouration of the integuments, but, as is usual in the more advanced stages, the ordinary changes of colour, indicative of the special injury, were extreme. A very remarkable case was fresh in his memory, where, from the kick of a horse in the scapular region, an enormous ecchymosis grew up in the situation of the injury, and within a few hours after the accident; no other injury was discoverable; and within the following twenty-four hours a profuse hæmoptysis was superadded, accompanied with dyspnœa and other chest symptoms. Now, a remarkable phenomenon manifested itself, in shape of an emphysematous-like crackling, on making firm and uniform compression over the extravasation. This sensation disappeared in proportion to the subsidence of the ecchymosed swelling, and ultimately complete recovery took

place. This peculiar phenomenon attendant on ecchymosed swelling, the result of injury, Mr. Fleming stated he had witnessed in very many instances, as he has elsewhere noted. He had found it under the scalp, on the trunk, over the several cavities, around the larger joints, over the region of the sacrum, and in the extremities. He was already engaged in placing such cases on record. The crepitus alluded to is sometimes very fine, so fine that it may escape superficial examination, is quickly obliterated by pressure, and again, equally quickly returns. It is similar to that delicate stethoscopic crepitus present in special fractures of the ribs, associated with pulmonary hæmorrhage, and, perhaps, produced by traumatic ecchymosis of the lung, or it resembles the very minute crepitation attendant on certain forms of pneumonia, or of cedema of the lungs. That this crepitating feel may be the local evidence of air in the areolar tissue, Mr. Fleming did not question, and admitted that this air may be secreted or generated in that tissue. Reflecting, however, upon the presence of the phenomenon in ecchymosed swellings, it did appear to him that the changes which the blood undergoes when it is extravasated through areolar tissue may, perhaps, in some measure account for it. Under such circumstances he thought that the blood is more or less quickly resolved into its rougher elementary constituents—that the walls of some of the spaces of the areolar tissue which contain those constituents, such as the serum and crassamentum, become as if glazed with this fibrinous element in the shape of plastic effusion—that other spaces completely filled with this will be partially obliterated, whilst again in other portions compartments will exist, which contain, in addition, in greater or less quantity, the crassamentary element mixed with serum. Thus, when the whole swelling is subjected to pressure, the physical sign alluded to is produced, though doubtless some chemical changes in the extravasated fluid, or a certain amount of local inflammatory action may also have their share in its production. Mr. Fleming begged to apologize for trespassing on the Society, but as traumatic pulmonary hæmorrhage was alluded to he hoped that these few practical remarks might be considered pertinent, and might not be devoid of interest in connexion with ecchymosis of the lungs, the result of injury—the attendant stethoscopic signs of which simulated so much those of the ordinary pathological lesions of the lungs as to lead to deception as to their cause.

DR. FINNY said he could not but feel highly flattered by the remarks that had fallen from the gentlemen who had spoken that evening. All the speakers were not in accord on the points he had brought forward, but he was glad to find that the majority was in favour of the views he had expressed, and he was justified in the inference that the opinion of the Society generally was with him, from the silence of many members who might have spoken had they held different views. Mr. Fleming's

allusion to traumatic hæmorrhage under the skin confirmed what he (Dr. Finny) had said. No doubt the crepitus that occurred in subcutaneous hæmorrhage was a phenomenon well known to surgeons, and indicated that there was set up some change of an inflammatory nature in the surrounding tissues. Similar changes were set up in the lungs, and he believed them to be products of inflammation, and this inflammation might go on to resolution and recovery, or to caseous pneumonia and phthisis. He had particularly avoided mixing up the questions of tubercle and phthisis. He himself believed in Niemeyer's view on that subject. Dr. Little had confirmed what he (Dr. Finny) had said, although he laid more stress upon the scrofulous diathesis than on the occurrence of hæmorrhage. He thought he could hardly be said to have laid less stress upon that condition than Dr. Little, for he expressly stated in his paper that the scrofulous or strumous diathesis was a factor of the greatest importance in the production of phthisis. Dr. Hayden asked what the strumous diathesis meant; and this question he answered himself, by saying that it was expressed by a general vulnerability and delicacy of the constitution; but he (Dr. Finny) could not go with Dr. Hayden when he said this extended to delicacy of the vessels of the lungs, and that in these cases, which formed the substance of his paper, pre-existent latent tubercle acting on the vessels of the lungs had caused the hæmoptysis, unless it were proved beyond doubt that such a delicacy of the vessels had existed prior to the bleeding. In his opinion, hæmorrhage into the lungs was not necessarily preceded by tubercles. Dr. Hayden, while he concurred in his views that pulmonary hæmorrhage might be a cause of phthisis, believed it must be copious. He (Dr. Finny) did not agree with him that the hæmorrhage must necessarily be copious. A small hæmoptysis might in some cases be sufficient to give rise to the disease; as, for example, in one of the cases he had brought forward, that of a young man, a very slight hæmoptysis was followed by phthisis, which ran its course in five months. In two of the cases thermometric observations were made, and this he considered of importance, as bearing on the question of inflammation. In one case, for the first four days, the temperature was moderate, about 100°, and on the 10th day it rose to 103°. In the second case the temperature, for five days, remained at 99°, but on the 7th day it rose three degrees, and continued at that height until the 17th day, when it fell again, and almost complete resolution took place, but, owing to the presence of the unresolved products of this inflammatory attack, it soon rose again. These observations clearly indicated that the hæmorrhage was not the result of inflammatory action, or of disease, involving destruction of the lung tissue; and that, when it had occurred, it was followed for only a few days by the usual febrile symptoms attendant on bleeding; but that, after the lapse of several days, the blood remaining in the smaller cells set up inflammatory action either

by decomposition or by its presence as a foreign body; and this inflammation, being of the type to which the term "scrofulous" has been applied, produced certain results of which the patients died, and owing to which the lung tissue broke down and ran into ulcerative cavities. It was to this group of phenomena he applied the word "phthisis" in his paper.

What he regarded as the strumous diathesis, which he believed with Drs. Little and Hayden to be so strong a factor, was that class of cases in which there was a tendency to an indolent inflammation and ulceration of the lymphatic glands by rapid cell-proliferation, in which, by the rapidity of their formation, and the pressure thus necessarily exercised upon each other, a retrograde or cheesy metamorphosis ensued. In like manner, hæmorrhage or any accident occurring to a lung of such a patient will produce a similar kind of inflammation, slow in its course, and throwing out certain products of inflammation which, in their turn, cause phthisis. Dr. Kennedy plainly stated that no case of hæmorrhage in a healthy man would lead to tubercle, and he quite agreed with him, for in his paper he did not allude to tubercle at all. It was very different in its relations to phthisis. Dr. Kennedy mentioned a case that rather confirmed him in his view that hæmorrhage might be the cause of hastening the process of destruction in a lung already diseased. He said his case had run, after bleeding, a rapid course of tubercle; and he (Dr. Finny) believed that hæmorrhage, occurring in a lung long diseased, might not only hasten the inflammatory changes, but also set up a tubercular deposit. As to hæmorrhage being rather a cause of blood poisoning than of phthisis, he did not think that was borne out by clinical observation. He had no doubt that blood, or the products of altered blood, might give rise to pyæmia in some few instances; but they were, he considered, quite the exception.

He had to thank Dr. M'Swiney for the manner in which he had treated his paper. The views of that gentleman had been already answered in the remarks already made. He adopted Dr. Williams's view of the fragility of the vessels as a cause of hæmoptysis, and Dr. Finny had mentioned that view in his paper, and had accorded it as much weight as he thought it deserved. Dr. Williams, he might mention, based his view on the microscopic examinations of Dr. R. Hall, but these observations were not made in lungs prior to death by phthisis; while the point Dr. Williams wished to make was that the vessels were diseased prior to any hæmoptysis, or any physical signs of disease. Dr. M'Swiney had referred to the treatment of hæmoptysis which had not been touched on in his paper, being merely mentioned in the details of the cases. He did not agree with that speaker or with Dr. Kennedy that it would be advisable to allow the bleeding to continue. It might be beneficial in some cases of congestive hæmoptysis, and they knew that Dr. Stokes's practice of bleeding from some other part, such as the arm,

had been found beneficial in some cases; but that, he thought, would not warrant them in allowing hæmorrhage to go on into a lung without trying to check it. It was assumed by some speakers that he entirely agreed with Niemeyer that hæmorrhage was a frequent cause of phthisis. He did not go so far, as he had plainly stated in his paper, but he held that it was occasionally a cause of phthisis, and as such he believed hæmorrhage into the lungs was at all times a serious phenomenon, and occasionally, under certain conditions, one fraught with the greatest danger to the patient.

The Society adjourned.

Wednesday, May 14th, 1873.

DR. GORDON in the Chair.

On Functional Mitral Murmur. By CHRISTOPHER J. NIXON, Licentiate of the King and Queen's College of Physicians, Ireland, &c.; one of the Physicians to the Mater Misericordiæ Hospital.

I purpose bringing under the notice of the Society some cases illustrating a point in cardiac pathology—the existence of mitral murmur independent of valvular lesion.

CASE I.—Henry Q., labourer, aged fifty, admitted into hospital on the 2nd of February last, complaining of dyspnoea. He told me that he had been a very hard liver; to use his own words, he “always drank as much whiskey as he could get.” Three months before admission he passed a large quantity of blood from the bowels; it was blackish in colour. At that time he suffered from the symptoms of gastric and intestinal catarrh, and shortly afterwards his belly became swollen. Owing to dyspnoea he was unable to follow his usual work, and attacks of lightness in the head, to which he had been subject for about a year, became more frequent, and caused him some alarm. Upon examination I found he had ascites; there was marked distension of the epigastric veins. The urine was scanty in amount, normal in specific gravity, faintly albuminous, and loaded with a deposit of lithates and purpurine. No tube casts could be found in it. Respiration was rapid and embarrassed, especially on the least exertion, and loud bronchitic râles were audible over the back of the chest. The impulse of the heart was feeble, and it extended over a large area. It was best marked a little to the left of left nipple, and about two inches below it. There was a great increase of cardiac dulness towards the left side. The sounds were well pronounced at apex and base; there was no murmur audible. The patient told me often complained of a sharp stinging pain in the cardiac regi-

attacked him suddenly, and then almost immediately passed away. There was no visible pulsation of either the arteries or veins of the neck. There existed the signs and symptoms of a weak and greatly dilated left ventricle; whilst from the history of the case and the nature of the man's habit of living, the ascites was thought to be due to commencing cirrhosis of the liver. I should mention there was no diminution of hepatic dulness and no evidence of splenic enlargement. It is also worthy of remark that there was not the least tenderness on pressure over the liver, and the patient never at any time complained of pain in the hypochondriac region, or in either shoulder. The usual remedies were employed to strengthen the heart's action, relieve the bronchitis, and promote the action of the kidneys and bowels. On the 10th of February, whilst examining the heart, I noticed, for the first time, a soft blowing murmur taking the place of the first sound. It was only to be heard in the horizontal posture, a little above the left nipple. At times it became inaudible; it returned then with a slight whiz, became more marked, and again disappeared. On only one occasion could the bruit be heard whilst the man was sitting up in bed. It was found necessary to administer a strong drastic cathartic. On the following day the patient complained that he felt very weak, and that the medicine had purged him very considerably. On examining the heart I heard, both in horizontal and vertical positions, the murmur, which was louder and more prolonged than usual, and on that day, for the first time, I was enabled to demonstrate its existence to the class. I should add the murmur was never heard in the axilla, or at the angle of the scapula; it was completely localized, and there was no accentuation of the pulmonic second sound. In a short time the dyspnoea became aggravated from the mechanical pressure of accumulated ascitic fluid, and from concurrent accession of bronchitis. The patient was unable to lie down in bed, and became exhausted from repeated attacks of vomiting which medicine failed to allay. The legs and scrotum became rapidly cedematous, and on the surface of the latter bluish-coloured patches made their appearance, as if gangrene were setting in. To palliate his sufferings and relieve his breathing I tapped the abdomen with Dieulafoy's aspirateur. He became so weak during the flow of the liquid that it was thought unadvisable to take away a very large quantity. Although improved and easier after paracentesis, I must say I think the relief came a little too late, as he sank in the latter part of the following day. Some hours before death I examined the heart, but the sounds were so masked by respiratory râles that I am unable to say whether the murmur existed or not. The condition of the heart was a matter of great interest to me to explain the cause of the intermittent murmur, and after a little trouble I was enabled to make the *post-mortem* examination. The liver was enlarged, its fibrous envelope greatly thickened, and on the under

surface of the left lobe and in the region of the transverse fissure it was cirrhotic. The spleen was of normal size, but its capsule was greatly thickened. The kidneys were granular on the surface, and in a state of incipient fatty degeneration. Both lungs were found congested at their bases. The pericardium was normal in appearance. The heart weighed eighteen ounces. The left ventricle was greatly enlarged and elongated, its apex being prolonged downwards for a considerable distance more than that of the right ventricle, which appeared markedly diminished in size contrasted with the left. The chamber of the left ventricle was immensely distended, and its walls were hypertrophied. The cavity of the right ventricle was much diminished in size from the encroachment on it of the septum ventriculorum. The muscular fibres presented on microscopic examination evidences of advanced fatty degeneration. The right auriculo-ventricular opening admitted the passage of three fingers, and the tricuspid valve was normal in appearance. The semi-lunar valves of the pulmonary artery and aorta were competent and free from disease. The left auriculo-ventricular opening was not dilated. It admitted only two fingers, and the curtains of the mitral valve were perfectly smooth, free from the slightest trace of deposit or thickening, and I believe perfectly competent. I regret I am unable to lay the exact measurements of the heart before the Society. By an accident the specimen was mislaid, I could not obtain the required measurements, or exhibit the case to the Pathological Society.

CASE II.—A boy, aged about fourteen, was admitted into hospital under my care, suffering from general malaise. I need not detail to the Society the history of the events of his illness, further than what is relevant to the subject matter of my paper. The case turned out to be one of tubercular meningitis, and as is usual in cases of this nature, the disease was ushered in by symptoms remarkably obscure. I first saw the boy whilst in the prodromatic stage, and as I felt considerable difficulty in diagnosing the nature of the case, I made a careful examination of both lungs and heart. The latter I found free from disease, as I could well judge from physical phenomena. The region of the cardiac dulness was normal in extent, and the impulse was felt in its usual place. The sounds were clear and free from murmur. During the prodromic stage of the disease, the stage of excitement gave place to that of depression. The pulse became slow, being reduced to about 60 beats in the minute. The temperature fell, so that the heat of the extremities had to be maintained artificially; the stupor became profound, and syncope and paralysis set in, and then I found the heart-sound was accompanied by a soft systolic murmur. The murmur was heard in its greatest intensity about the apex of the heart, and was audible above

point. At first I thought it was pulmonic in origin, but a further examination convinced me that it was a mitral murmur, as it was heard much more distinctly towards the apex than towards the base. It was audible in the axilla, not in back or towards the ensiform cartilage. It appeared to follow more than to accompany the first sound, and it was somewhat louder in the horizontal than in the vertical position. There was no alteration of the second sound at the base. I called the attention of my colleague, Dr Hayden, to the murmur, and he recognized it as a mitral regurgitant one. The murmur continued unaltered in character up to the time of the boy's death, which took place about a fortnight after the bruit had been first detected.

The organs were shown recently at the Pathological Society. They were examples of acute general tuberculosis, and the brain presented the appearances usually noticed in cases of acute hydrocephalus. The heart was perfectly normal in structure, shape, and size. The cavities and orifices were of the usual dimensions; its valves were competent and healthy in appearance. Not the faintest trace of deposition or alteration of structure could be seen on the mitral segments, and their edges were thin and smooth. At the meeting of the Pathological Society, I expressed my inability to account for the production of the murmur, except that it might be attributed to progressive debility of the muscular structure of the heart occurring in the course of the disease. There are special points of interest in this case worthy, I think, of observation. The occurrence of apex murmur in a case of cerebral disease, where the vitality of the system was much lowered, bears out the observation of Da Costa, that in those instances the cause of functional disorder of the heart is due to defective innervation. It is probable that we may trace to a similar source those cases of typhus fever, and other adynamic conditions of the heart, where the first sound at the apex is replaced by a murmur.

Moreover, in this case the bruit was developed, not in an excited state of the circulation, when the physical effects of increased friction might be productive of murmur, but when the beats of the heart were infrequent, being only 60 in the minute.

CASE III.—A boy, aged nineteen, came under observation on the 20th of February last. He was suffering from aggravated emphysema and bronchitis. He had been ill since his childhood he suffered from short breathing, and (recently of late he experienced paroxysmal attacks of dyspnoea, occurring at the least exertion, and causing him to lean against something for support whilst he gasped for breath. The circulation was greatly embarrassed; his feet were cold and cedematous; the lips bluish. The heart was displaced downwards, and its impulse beat high up in the sternum. The right cavities were dilated; there was a bruit heard in the neck. The liver was enlarged from

surface of the left lobe and in the region of the transverse fissure it was cirrhotic. The spleen was of normal size, but its capsule was greatly thickened. The kidneys were granular on the surface, and in a state of incipient fatty degeneration. Both lungs were found congested at their bases. The pericardium was normal in appearance. The heart weighed eighteen ounces. The left ventricle was greatly enlarged and elongated, its apex being prolonged downwards for a considerable distance more than that of the right ventricle, which appeared markedly diminished in size contrasted with the left. The chamber of the left ventricle was immensely distended, and its walls were hypertrophied. The cavity of the right ventricle was much diminished in size from the encroachment on it of the septum ventriculorum. The muscular fibres presented on microscopic examination evidences of advanced fatty degeneration. The right auriculo-ventricular opening admitted the passage of three fingers, and the tricuspid valve was normal in appearance. The semi-lunar valves of the pulmonary artery and aorta were competent and free from disease. The left auriculo-ventricular opening was not dilated. It admitted only two fingers, and the curtains of the mitral valve were perfectly smooth, free from the slightest trace of deposit or thickening, and I believe perfectly competent. I regret I am unable to lay the exact measurements of the heart before the Society. By an accident the specimen was mislaid, I could not obtain the required measurements, or exhibit the case to the Pathological Society.

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point. At first I thought it was pulmonic in origin, but a further examination convinced me that it was a mitral murmur, as it was heard much more distinctly towards the apex than towards the base. It was audible in the axilla, not in back or towards the ensiform cartilage. It appeared to follow more than to accompany the first sound, and it was somewhat louder in the horizontal than in the vertical position. There was no alteration of the second sound at the base. I called the attention of my colleague, Dr Hayden, to the murmur, and he recognized it as a mitral regurgitant one. The murmur continued unaltered in character up to the time of the boy's death, which took place about a fortnight after the bruit had been first detected.

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congestion. The sounds of the heart, both at base and apex, were clear, well pronounced, and free from murmur.

On the eighth day after admission, the first sound was replaced by an exceedingly loud and rasping bruit, audible about the region of the apex, in the axilla, and at the angle of the scapula. It was accompanied by fremitus. It was not heard distinctly in the epigastrium, or over the ensiform cartilage. During auscultation the murmur changed in character. It became at times so faint as to be barely audible, and sometimes it disappeared entirely, returning with a loud whiz. It was intensified in the horizontal position. I cannot say that the second sound over the pulmonary artery was accentuated. As the murmur was absent in the heart for eight days following admission, and as it presented such varying acoustic characters, I did not believe that it indicated organic disease of the mitral valve, and supposed it to arise from functional causes. Its site, and its being audible in the axilla, and at the angle of the scapula, indicated that the murmur was produced at the mitral orifice. The boy did not improve under treatment; his aspect became more cyanotic; his abdomen ascitic; his legs and thighs enormously oedematous; the bases of both lungs were dull and crepitating; the expectoration became mixed with blood, and he died on the 26th of March. I exhibited the lungs and heart to the Pathological Society on that day. Both lungs were advancedly emphysematous and congested at their bases. The heart had the square form characteristic of an enlarged right ventricle. It weighed twelve ounces. There was no distinct apex; what would represent it was formed by the right ventricle. The surface of the heart presented a mottled appearance, due to sub-pericardial fatty deposition. In the interior of the right ventricle, the muscular column, especially those of King, presented numerous foci of fatty degeneration, occurring in buff-coloured patches visible through the endocardium. The pulmonary artery was dilated, and presented traces of atheromatous deposition. The left ventricle appeared normal in size. In only one spot did it present the mottled appearance observed in the right ventricle, viz., on the musculus papillaris attached to the posterior segment of the mitral valve. The fibres of the ventricle presented microscopic evidence of fatty change. The segments of the mitral valve were normal in appearance; no trace of thickening at the edges, and apparently perfectly competent. The following are the measurements of the heart:—

	Inches.
Length of left ventricle, from aortic orifice to apex,	3 $\frac{4}{10}$
Thickness of walls, at base,	$\frac{6}{10}$
Width of aortic orifice,	2 $\frac{2}{10}$
Do. of left auriculo-ventricular orifice,	4 $\frac{6}{10}$
Length of right ventricle, from pulmonic orifice to apex,	3 $\frac{8}{10}$

	Inches.
Thickness of walls at base,	$\frac{9}{16}$
Width of pulmonic orifice,	$3\frac{4}{16}$
Do. of right auriculo-ventricular orifice,	$5\frac{9}{16}$

Apart from the interest attached to the case on account of the mitral bruit, the state of the right ventricle and pulmonary artery is worthy of observation, as illustrative of the effects of over-strain on the tissue of the heart. Judging from the history of the case, the boy probably suffered from emphysema since childhood. The continued obstruction to the pulmonic circuit of the blood eventuated in permanent distension of the pulmonary artery and right ventricle, and, in its turn, this permanent distension led to the degenerative changes found.

CASE IV.—Bridget Gillooley, aged forty-six, the mother of eight children, was admitted into hospital on the 7th of April last. The history of her illness, as obtained from herself, is as follows:—She complained for about eighteen months of lightnesses in the head, failing strength, and palpitation of the heart. She had no cough, and never had rheumatic fever. In August last she had a miscarriage; the quantity of blood lost was very great, and several fainting fits were induced. She was confined to bed for several months suffering from anæmia, and not regaining her strength she came to hospital. When I saw her she presented the aspect of a woman who had lost much blood. Her lips and cheeks were blanched; the sclerotics were pearly white; the matrix of the nails pallid. She complained of weakness, beating of the heart, and disordered vision. Pulse 72 whilst sitting up in bed; 66 in the horizontal position. It was occasionally intermittent, but fairly regular in its beats. Its volume varied considerably; it was at times full and strong, and at other times soft and weak.

There was no cough or respiratory distress, although the vesicular murmur over the back of both lungs was much rougher than usual. The apex beat of the heart was in its usual position. There was no increase of cardiac dulness. In the horizontal posture the external jugulars were seen to be distended. There was visible pulsation in the carotids and subclavians. On stethoscopic examination a venous hum was heard in the external jugulars, and the least pressure over the arteries of the neck developed a bellows murmur. Upon listening over the heart a faint systolic bruit was heard in the aorta, about the second costo-sternal articulation of right side. At the apex I discovered a soft systolic bellows murmur. It was heard somewhat more intensely about an inch and a-half above the impulse beat, and a little to the right side. It was localized; not being audible in the axilla, at the angle of the scapula, or at the ensiform cartilage. It was specially well-marked after making a

forcible inspiration. It varied greatly in intensity; at times there would seem to be merely a prolongation of a dull heavy first sound, but a few more contractions of the ventricle brought out the clear blowing whiz. Frequently one or more abrupt rapid contractions of the ventricle took place, producing an irregularity of rhythm followed by an intermission of the heart.

Comparing the radial pulse with the action of the heart, I found the irregular contractions of the ventricle but feebly represented, and each time the bruit became intensified there was a corresponding diminution in the volume of the pulse. This observation I verified by repeated examinations. The second sound of the pulmonary artery seemed to me to be faintly accented. I now placed the patient sitting up in bed. The venous engorgement of the neck subsided, and also the visible pulsation of the arteries. The feeble basic murmur was heard over the aorta, but the murmur at the apex was no longer audible, and the sounds were fairly well-pro-nounced. I placed the patient again in the recumbent posture, and in a short space of time, corresponding to about thirty beats of the heart, the mitral bruit again returned, presenting the same characters, whilst the distension of the jugulars and the pulsation of the arteries of the neck again became visible. Frequently since the patient's admission, I have heard the mitral murmur, when the action of the heart was very feeble, in the vertical position. The mere exertion of getting out of bed entirely removed it, and the sounds of the heart became clear, loud, and free from murmur. At the present time, I may say, the features of the case in many respects are similar to those described, and the woman is still under observation. The basic bruit has, however, entirely disappeared under treatment, and the patient has become much improved both in appearance and strength. The apex murmur is still audible in the horizontal position, but is becoming more faint and abbreviated in character.

It is a question whether the bruit in this instance is associated with some lesion of the texture of the heart; or, is it simply due to the hydræmic condition of the blood? There is no evidence of passive dilatation of the left ventricle, nor were the symptoms complained of previous to the loss of blood sufficient to warrant one in diagnosing that fatty degeneration of the heart then existed. How far the anæmia is the direct cause of the murmur it would be hard to determine. It is interesting to observe, that it produced its characteristic bruit, "basic in seat, and systolic in time," and when the condition of the patient improved, and the plasticity of the blood was restored, the aortic murmur ceased to exist, whilst the mitral still remained.*

CASE V.—This case only requires a few words. It is an instance of

* At the present time (May 24th) the patient is, to all appearances, well. The mitral bruit has entirely disappeared.

mitral murmur developed during fits of severe palpitation of the heart. A lady, aged twenty-one, complained that since childhood, but especially during the past two or three years, she was subject to irregular action and palpitation of the heart. The points of interest in the case are as follows:—For six or eight hours preceding the attack of palpitation, the action of the heart became irregular, it occasionally stopped, producing a peculiar feeling of uneasiness, whilst now and then a fluttering sensation was felt in the cardiac region. By these sensations an attack of palpitation could be anticipated. When these precursory symptoms had lasted for the time mentioned, the heart suddenly commenced to beat violently, the inordinate action being accompanied by a jerking movement of the head corresponding to the cardiac contractions. I frequently listened to the heart during a paroxysm of palpitation. The beats counted 180 in the minute, and they were regular in time. The pulse at the wrist became extremely feeble. At the apex of the heart a short rough murmur was heard; there was no murmur at the base. From the frequency of the heart's action, it would be difficult to say what sound the murmur accompanied, but from its rhythm, I believe it to be the first. The attack of palpitation lasted on an average about ten minutes, and as suddenly as it was ushered in, so it suddenly ceased, the cessation being attended with a violent shock in the motion of the heart, which was felt by the patient. Immediately the slow, regular action of the heart was resumed, the pulse fell to 70, and the apex murmur entirely vanished. These attacks of palpitation were frequently brought on by mental distress or anxiety of any kind, and always after drinking green tea.

The case, evidently, is one of neurosis of the heart. The existence, with the other signs and symptoms, of mitral bruit during attacks of palpitation, warranted me in briefly bringing it under the notice of the Society.

The similarity in character of a functional bruit to one arising from organic disease, and the uncertainty which exists as to its mode of production, and its site of development, necessarily involve the study of inorganic murmur in much obscurity. Notwithstanding the experiments and observations of Bouillaud, Skoda, Hope, Corrigan, Chauveau, and others, Marey—no mean observer—states that “no one has, up to the present time, given a really scientific demonstration of the causes of blowing sounds.” Nor is the site of functional murmur even as yet a settled point. The older teachings of Hope and Beau, led us to believe that it was developed at the aortic orifice; whilst Dr. Hughes, in his paper on “Anæmic Murmurs and their Diagnosis,” holds that they are produced in the pulmonary artery. Again, more recently, Parrot, in a remarkable and elaborate article, in the *Archives Générales de Médecine*, labours to prove that the murmur is developed at the right auriculo-ventri-

cular orifice, that it is always accompanied by a venous pulse in the external jugulars, and that it is indicative of tricuspid insufficiency.

Moreover, according to this author, the murmur is not only found in anæmia and chlorosis, but it also occurs in certain cachexies, hypochondriasis, hysteria, in most cases of acute articular rheumatism, and in fevers. The remarks of Parrot are worthy of much consideration, and they bear upon the cases which I have just recorded. In six cases of systolic apex murmur observed by Flint, *post-mortem* examination revealed no organic change in the curtains of the mitral valve. In all of these cases, save one, Dr. Flint believes that the murmur heard was a tricuspid apex one, and he urges that in many cases of functional bruit a like explanation will apply. In some of Dr. Flint's cases, emphysema and cyanosis existed, and in one case, where the murmur was both loud and rough, the walls of the right ventricle were nearly as thick as those of the left. In other cases of apex murmur, Dr. Flint suggests that the murmur might be due to the pressure exercised by the apex of the heart, in its systolic movements, against the tongue of lung which projects over it, the movement forcing the lung against the thoracic wall, and expelling the air from the cells with sufficient force to produce a blowing sound. It is obvious, however, that even if it be conceded that a murmur could be developed under such conditions, it would accompany not the sounds but the motions of the heart, and should therefore be recognized as exocardial. Bearing in mind the proximity of the auriculo-ventricular orifices to each other, and the alteration in position and shape which the heart, from various causes, may undergo, it must be always a matter of difficulty to discriminate with accuracy tricuspid from mitral murmur, except, indeed, we accept the dictum of a distinguished writer on cardiac disease, and exclude altogether from diagnosis valvular lesions of the right side. But granting, that in some cases apex murmur is indicative of tricuspid insufficiency, it cannot but be believed from the recorded observations of Gardiner, Bristowe, Hayden, Da Costa, and others, that in the vast majority of cases of functional apex murmur, its site of development is at the mitral orifice, and the high authority of Drs. Stokes and Walshe may be cited, for the possibility of the occurrence of mitral murmur, in a dilated and hypertrophied condition of the left ventricle. Moreover, in some of the published cases, the mitral valve, though healthy in appearance, was found insufficient to close the greatly dilated auriculo-ventricular orifice, and certainly here, the regurgitation which must have taken place explained the existence of mitral murmur. But where the curtains of the mitral valve are proportionate in size to the orifice which it guards, how are we to account for the production of murmur?

I exclude from consideration those cases where the valves appear of normal size, but are thickened at their edges. In those instances, murmur is believed by Oppolzer to be due to an arrest of the normal vibrations of

the valve, though it would certainly occur to one's mind, that those changes in the valve, by destroying what corresponds to its lunula, would render the perfect closure of the two curtains incomplete and inefficient. This view would appear to be borne out by the experiments of King, who showed that by injecting the left ventricle from the aorta, in hearts where the segments of the mitral valve were opaque and thickened, a fine jet of regurgitation could be observed taking place through the mitral orifice, viewing it through the previously opened left auricle.

To return to the question, as to how functional bruit is produced independent of any change in the structure of the valve, we must accept one of two explanations. Either, according to Bamberger, the murmur is due solely to sonorous vibrations of the auriculo-ventricular valves, produced by their want of tension, owing to fatty degeneration of the papillary muscles; or, it is due to regurgitation of blood through a functionally imperfect valve. It would be idle to detain the Society by narrating arguments in support of the former view. Ingenious experiments, showing the effects of fluids of different specific gravities in active motion, and their action upon membrane in different states of tension, have been recorded. Bergeon has shown that a sudden shock may throw the molecules of a fluid into active vibrations, and that these may form a part cause of murmur. But if it were to be believed that mere defect or excess of tension of membrane, in certain states of the blood, was sufficient in itself to generate a *bruit de soufflet*, why is it that we never have a diastolic basic murmur due to altered tension of the semilunar valves of the aorta or pulmonary artery? It is true the anatomical connexions and surroundings of the semilunar valves and the auriculo-ventricular are very different, but pathological experience and morbid anatomy show us, that in some cases states arise which must lead to considerable variation in the tension of the semilunar valves, and in which no abnormal sound is developed.

I believe, then, I would be borne out in saying that most authorities agree in the axiom of Bristowe, that "a systolic apex murmur is a positive proof of regurgitation through the mitral orifice."

The absence of the sound of Skoda, so frequently developed in organic disease of the mitral valve, and the non-existence, as a rule, of any signs or symptoms of pulmonary distress, should not, I think, be regarded as valid arguments against the theory of functional regurgitation. The amount of blood thrown back into the auricle is, in all probability, small, and taking into consideration the reservoir powers of the left auricle and the veins leading to it, it is not unreasonable to argue, that in an early stage the effects of the regurgitation on the pulmonary circulation would be so small as not to give rise to any serious disturbance of the respiratory function, or produce physical evidence of its results. But should the regurgitation be protracted in duration, it would be impossible not to believe but that pulmonic complications would ensue.

Assuming then that the murmur is the symbol of regurgitation, it becomes necessary to explain, if possible, its mode of production. Analysing the different states in which mitral murmur functionally arises, I may be justified in classing them as follows:—

Firstly,—It may occur, and be constant in duration, in dilatation of the left ventricle associated with hypertrophy, or in simple dilatation of the left chamber. In both of these instances advanced degenerative changes of the myocardium will, I believe, be generally found.

Secondly,—Where the auriculo-ventricular orifice becomes so dilated as to render the valves incompetent.

Thirdly,—In adynamic conditions of the system, leading to deficient innervation of the heart, as in cases of cerebral disease, and in those cases of low typhus fever recorded by Dr. Stokes, where the first sound at the apex was replaced by a soft blowing murmur.

Fourthly,—In certain neuroses of the heart which produce an irregular action. In this class I would include some cases of heart complication occurring in chorea, and those due to excessive use of tea and tobacco.

And fifthly,—In those cases which have been described by Drs. Hayden and Da Costa, where the murmur was due to an irritable condition of the heart, with palpitation, or traceable to various conditions of the system, as anæmia, purpura, nervous debility.

Instances where murmur arises from the presence of a clot in the cavity of the ventricle, and where it is due, as it is said, to obstructed pulmonary circulation, are not included in these classes.

Taking into consideration these varying conditions, it is not, I think, possible to offer any single explanation as to the cause of murmur which will be applicable to all. In the cases recorded by Bristowe, many of which probably should be given as instances of organic disease, there was a disproportion between the cavity of the ventricle and the length of the cordæ tendinæ and musculi papillares. Again, Dr. Hare's explanation, that in cases where there is great dilatation there is a lateral displacement of the origins of the papillary muscles, and consequently an alteration in their axis of action, can only be applicable where dilatation exists. Is it not more likely, that at least in the left ventricle, the displacement is more a centrifugal than a lateral one, considering that the dilatation involves all the walls of the ventricle? Dr. Hayden, without pledging himself to a definite theory, believes that during the centripetal movement of the walls of the ventricle a yielding of a portion of the wall outwards may take place, carrying with it the origins of one or both papillary muscles, preventing their movement of interlocking, and so inverting their function as to make them active agents in divaricating the valves; hence regurgitation takes place. This view is an important one, as it refers the production of the murmur to an atonic condition of the muscular

structure of the heart itself, and I would hazard the opinion, that any explanation which has for its basis entirely physical conditions as the sole productive cause of functional mitral murmur will be found insufficient. There may be some truth in what has been said, that there is too great a tendency in modern pathologists to account for what may be called vital phenomena by physical or mechanical conditions. Many speculations built upon applied mechanical laws have been dissipated by time and observation. The distinguished writer of the article "on the physical basis of life," whilst he propounds the doctrine that "all vital action is the result of the molecular forces of the protoplasm which displays it," must himself have seen that his suppositions did not clear away the mist, as there was a mere substitution for one indefinite term of another equally so. In the same way how many phenomena sought to be accounted for by physical conditions still leave us in doubt as to their origin and development? The cause of the contraction of the chambers of the heart is still unknown, and the theories of v. Bezold and of Paget, if they can be regarded as affording an explanation, still only remove a difficulty to create a greater one. If there be an inhibito-motor nerve and an excito-motor, by what means is force regularly supplied to each? When we become believers in rhythmic nutrition, we have to ask ourselves how it is brought about? But whether the power of rhythmic contraction is an inherent property of the muscular structure of the heart, or is derived from its intrinsic nervous system, still we must admit that this power is not a tangible substance, which we can make subservient to mechanical laws, or which partakes of the "affections of matter." So that, vague and indefinite as the term "vital force" may be, still, it is probable that it is only under this name we shall ever be able to explain conditions or actions which we commonly speak of as vital. If we study the condition of a ventricle during a single contraction, we find that immediately after distension the act of shortening commences, and the valves are closed from the pressure of the blood against their surface; at the same time the papillary muscles contract, interlock with each other, and draw the segments of the valve to opposite walls of the chamber, pulling upon the cordæ tendinæ in such a way as to twist their extremities and the edges of the valves around each other, forming an irregular spiral cone, the apex of which hangs down into the cavity of the ventricle. By this process the edges of the valve are, as it were, sealed to each other, and the shortening of the fibres of the ventricle continues until the chamber has entirely emptied itself of blood through the arterial outlet. In order then to ensure a healthy or normal contraction, it is evident there must be a perfect unity or correspondence in action of the fibres of the walls of the ventricle and of those which are connected to the valves; and moreover, the peculiar mode of closure of the latter must be maintained to secure a thorough and complete closure of the auriculo-ventricular septum. The

only way in which we can account for this spontaneity of action is by referring it to the vital condition or power of the heart itself. But is it not possible that external influences, disease, altered nutrition, or disturbed innervation, might alter this condition in such a way, that the correspondence in action of different movements might be disturbed, and a different result attained? Organs not endowed with such complexity of structural arrangement as the heart, present in disease instances of want of unity or correspondence in action of component parts. For instance, in atony of the bladder we may have the circular fibres of the sphincter alone paralysed, whilst the fibres of the body of the bladder are unimpaired in function; or, as in fevers, the retentive power of the bladder may be unaffected, whilst its expulsive power is for the time destroyed. Again, the pathology of Ileus affords us an instance of want of uniformity of action of the muscular fibres of gut; and in the voluntary muscles, after prolonged violent exertion, a disorder of function is evidenced by the tremors which are occasionally observed in the muscles employed.

I would then, with great diffidence, suggest that in most cases where functional mitral murmur is developed, it is the result of an irregularity, or rather, a want of correspondence in the action of the sets of fibres of the ventricle, which obliterate its cavity, and those which close its valve; that this altered function is entirely due to some defect in the vital power or condition of the heart itself, which leads either to atony of the papillary muscles or derangement in the rhythm of their movement.

It only remains to allude briefly to the points of diagnosis, which might distinguish a mitral bruit of organic origin from one due to functional causes. In the latter the changeable character of the murmur and its alteration by position is of much importance. In some cases it is inaudible in the vertical position, and in almost all it is intensified in the horizontal. In those cases where there is an irritable heart, with a quick excited action, it may only be developed after vigorous exercise, or from mental excitement. Da Costa makes the curious observation, that a murmur from mere temporary excitement of the heart can be developed in at least one out of every twenty healthy persons. On the other hand, in conditions of the heart where we have reason to believe that its muscular structure is impaired in strength, or the seat of degeneration, the murmur may be developed only when the movements of the heart are slow and feeble; but when it is roused to vigorous action, as in making any forcible effort, the murmur may cease to exist. In nearly all cases where it is constant in existence the murmur will present varying intensity of note, sometimes being barely audible, at other times loud and well-pronounced. So that the varying existence of the murmur and its varying intensity are important diagnostic points. The rise and fall of the radial pulse I alluded to in the case of the woman Gillooley; it has

been also noticed by Dr. Hayden in the cases of functional mitral regurgitation recorded by him. It has been stated that in organic mitral disease we can approximately measure the amount of regurgitation by the size of the radial pulse; in cases of functional regurgitation the changing volume of the pulse would serve to point out when reflux took place or ceased. This variation in volume of the pulse does not take place in regurgitation depending upon organic disease of the mitral valve: the physical conditions which originate the murmur remaining unchanged there is consequently no alteration of arterial tension.

The almost general absence of pulmonary distress, and of accentuated second sound, or of doubling of the second sound—phenomena so frequently associated with organic mitral disease—are important diagnostic points. But, as I before stated, should the regurgitation continue long in existence I thoroughly agree in the observation of Dr. Cuming, that it would be “impossible to comprehend why a regurgitation from functional derangement of the valve should not be accompanied or followed by the same disturbance of the circulation as would be brought about by a similar amount of regurgitation owing to actual changes in the structure of the valves.”

In cases associated with the constitutional effects mentioned as nervous debility, purpura, anæmia, &c., there will be no alteration in the position or size of the heart, as evidenced by palpation and percussion. In such cases, unless we believed the bruit to be the result of recent endocarditis, the non-existence of increased dulness over the heart, or displacement of its apex, would be taken as favouring the view of functional lesion.

Functional mitral murmur differs, it is said, from organic, in being more localized, and that it is not heard either in the axilla or at the angle of the scapula. The localization of a murmur and its transmission depend greatly, I take it, upon its intensity of note, and if it be sufficiently well-pronounced a functional murmur will be as diffused and as capable of transmission as an organic one.

A point which is much insisted upon by Da Costa is, that the murmur is heard at its point of greatest intensity, not at the apex of the heart, but somewhat above it, over the body of the ventricle, and in the cases which I observed, I can bear out the truth of this statement. I am unable to offer any explanation for this—the view that the left auricle conveys the sound to the chest wall ought to apply in cases of organic disease where there is reflux as well as to functional bruit.

The frequent absence of anæmia in cases of functional mitral bruit, suggests the idea, that it is oftener a complication than a direct cause. I would barely hint, that in the majority of those cases where anæmia or hydræmia exists, and is the indirect cause of functional mitral bruit, an aortic or pulmonic systolic murmur will also be developed. The case of the woman Gillooley may be cited as affording some grounds for this

presumption. In this case I remarked that the murmur was influenced by the respiratory function, being heard most distinctly after a full inspiration, but whether this phenomenon existed or not in the other cases I am unable to say.

After the many able memoirs which have been published on functional murmurs, I confess I felt some diffidence in introducing the subject anew to the Society; but as I had been unusually fortunate in having the opportunity of studying the features of the cases which I have the honour of bringing under your notice, I thought it would be legitimate on my part to record them, for the sake of eliciting your opinions as to their import. I have, moreover, a very strong impression that inorganic mitral murmur is of much greater frequency than is commonly taught or believed, and that the old doctrine, that murmurs are the landmarks of valvular disease, needs much qualification, especially when applied to those developed of the apex of the heart.

The accumulation of fresh facts strengthens or weakens doctrines already propounded, and in the ever-changing cycle of human knowledge facts alone stand the test of time. A desire then to add a fragment in confirmation of much of what has already been ably dealt with, must be my apology for occupying so much of the time of this Society.

The CHAIRMAN congratulated Dr. Nixon upon his most interesting and exhaustive paper. The division of his cases was a most practical one—first (the title of his paper having excluded valvular disease), where there was valvular deficiency in consequence of alteration of shape or length of the ventricle, frequently found in hypertrophy, produced by Bright's disease or other causes, and, as he believed, originally described by Dr. Gairdner; and secondly, that most interesting class of cases in which, although a mitral murmur (unassociated with any other physical sign of cardiac disease) existed for a time, it vanished without leaving any signs of heart disease, or if the patient died (as had sometimes occurred), without any abnormal appearance being detectable. This latter class was usually connected with some nervous lesion, and its co-existence with other symptoms might almost warrant the conclusion that they had their origin in nervous lesion. Thus, he might adduce the case of a growing boy whom he saw three years ago, suffering from an attack of hay asthma, and the attack was accompanied by a loud mitral murmur. This boy had for many years been under his care, and he was enabled to state that this was the first occasion in which the mitral murmur was developed. He recovered from the hay asthma, and the murmur disappeared. The second year the same phenomenon occurred, an attack of hay asthma, with temporary mitral murmur, and the third year it occurred also. Each year he had an attack of hay asthma, and with it

there was a loud, rough, rasping, regurgitant murmur, which totally disappeared when the hay asthma went off.

DR. HAYDEN had hoped that this very interesting communication would have elicited the opinion of some of their distinguished *confrères*. For himself, he could say that he had for many years taken a very deep interest in this subject. He needed only to advert to the importance with which it was invested with regard to the insurance of life. A few years ago it was an almost universally accepted dogma that, given a case of veritable apex systolic murmur, that case was not insurable on ordinary terms, inasmuch as it was supposed to imply organic disease of the mitral valves. When he first entered on the study of cardiac disease this was also his impression, and continued to be so for many years. He was first awakened from the delusion in which he had previously been by the occurrence of one or two cases of a very singular kind. The first of these was that of a man who had been an excessive tobacco smoker. He was universally tremulous, nervous to a degree, pallid and tottering. On examining the heart he discovered a systolic murmur at the apex. He put him under treatment on the assumption that he had organic mitral disease. He treated him with iron and bromide of potassium, and in the course of a week or two the murmur ceased to be heard, and the man was cured *quoad* the heart. Other cases presented themselves in rapid succession, some being examples of masturbation. The general character of the individuals indicated nervous and muscular debility; they were tottering, extremely nervous, tremulous, sometimes stammering or inarticulate in speech. In these cases, likewise under treatment, the murmur ceased to be heard. A few years ago a case of a very singular kind came under his notice. It was that of a man suffering from fatty disease of the heart. He was generally anasarcaous, had enlargement of the liver, the pulse was irregular, the heart weak, the sounds all but suppressed, but at the apex of the heart there was a distinct blowing systolic murmur, and he came to the conclusion that there was organic disease of the mitral valve. To his utter discomfiture, when the man died the mitral and all the other valves were discovered to be perfectly sound, but the left ventricle was soft and in an advanced stage of fatty degeneration. That case made such an impression upon him that he turned his attention to the subject of non-valvular mitral murmur of systolic rhythm, and the result was a paper which he had the honour of reading before the British Medical Association in 1867. The view he took of these cases was, as Dr. Nixon had stated, that the murmur was due to the yielding of a portion of the left ventricular wall, in consequence of which the papillary muscles were altered in their relation to the valves; that is to say, whereas in the healthy state the ventricular walls mutually approximate in contraction till, at the acme of systole the papillary

muscles which are so placed on opposite sides of the cavity as to interlock, have actually changed places, or passed reciprocally to opposite sides, and drawn the tendinous chords attached to their extremities in the same direction, so as effectually to close and make tense the valves, this centripetal movement of the ventricular parietes cannot take place if they be generally enfeebled, or even if they be only partially so, the weak portion being that to which one or both papillary muscles are attached. At the acme of systole, this portion of the wall yielding under the eccentric portion of the blood, will move outwards, and carry with it the attached papillary muscles, and so raise the valve and permit reflux. He did not bind himself to this doctrine, but he thought, in the absence of a rational explanation of the facts, they were justified in propounding a hypothesis, the unquestionable facts being, that mitral regurgitant murmur has frequently occurred in the absence of disease or inadequacy of the mitral valve, and that such murmur has been always associated with debility of the walls of the left ventricle. He was not satisfied with Dr. Nixon's explanation of its production by neurosis. Dr. Walshe had alluded to this as an explanation of the occurrence of mitral murmur in cases of chorea; but he also showed the difficulty of accepting that explanation, for there was no irregularity of rhythm in the pulsation of the heart, and it was difficult to assume, while that was so, that the papillary muscles, which are in direct continuity with the walls, as shown by Bell Pettigrew, could be acting irregularly and out of harmony with the walls themselves. He did not think analogy could be drawn from the example adduced by Dr. Walshe of fascicular spasm in voluntary muscles, because in these muscles the fasciculi are independent entities, whereas in the heart such is not the case. Thus, in a voluntary muscle one set of fasciculi may be in action while the others are at rest, and in ordinary contraction such is actually the case, and hence the *bruit musculaire*; but in the heart, the fibres of which are ramified and interlaced, and directly continued into those of the subjacent layers by involution, there can be no partial contraction. The systole of each ventricle engages all its component fibres simultaneously, including those of the papillary muscles, which must, therefore, act in harmony with the remainder. In most of the examples of this murmur which he had seen there had been strong evidence of muscular debility. In cases where there was not organic softening of the heart, there was temporary atony and instability. These were the curable cases, and it was in reference to these that the importance of diagnosis was to be considered. As regarded the differential diagnosis, he did not consider it difficult. As a rule, in those cases in which there was mitral murmur without organic lesion there was great debility of the nervous system. There was usually no irregularity of the pulse, but only a variation in its rate at brief, but unequal, intervals, from nervous excitement without

adequate cause. Pulmonary congestion and its consequences, dyspnœa, cough, expectoration, and dropsy, were likewise absent, because regurgitation is temporary, and the pulmonary vessels are capable of resisting temporary engorgement. Then there was in the murmur itself a certain quality which should be borne in mind; it was an exceedingly soft blowing murmur; it did not supersede the first sound, but accompanied or succeeded it. He thought, having regard to all these features, they might arrive in nearly all, if not in all, such instances at a positive and correct diagnosis.

DR. HENRY KENNEDY said the subject so very ably brought before them by Dr. Nixon was too large to enter into it at length. He considered that the production of a souffle, even in the absence of valvular disease, was often originated by more causes than one; and that these co-existed. Thus there might be a disproportion between the capacity of the ventricle and its opening, at the same time that the texture of the organ or its *cordæ tendinæ* were degenerated. In all the soft or blowing murmurs to which allusion had just been made by Dr. Hayden, Dr. Kennedy believed that the state of the blood had much to say to their production; otherwise it seemed to him impossible to account for the presence of a murmur in certain cases of fever, and its disappearance in the course of two or three weeks—a point first noticed by Dr. Stokes. He was glad to find, too, that the author had confirmed an observation relative to the changes which soft murmurs especially undergo, according to the position of the patient. In a paper which Dr. Kennedy had brought before the Association a few years back, he had detailed a number of cases in which a single souffle was either entirely lost, or rendered much weaker when the patient assumed the upright position. He considered the point one of great practical importance, which still afforded a wide field for investigation, and at the time he wrote the paper had received little, if any, attention in the standard works on cardiac diseases.

DR. NIXON briefly thanked the Society for the manner in which his paper had been received.

On the Influence of Digitalis on the Weak Heart of Typhus Fever. By THOMAS WRIGLEY GRIMSHAW, M.D., Fellow and Censor of the College of Physicians; Physician to Dr. Steevens' Hospital, and Cork-street Fever Hospital, &c.

ON the 8th of January my friend Dr. Little brought under the notice of the Society the subject of the use of digitalis in the failing heart and delirium of acute disease. On that occasion the author appealed to those present who had the opportunity to test the value of the drug in the

treatment of the adynamic affections of fever. I determined to adopt Dr. Little's suggestion, and to make the trial in any suitable cases which might come under my care. It was suggested by my former colleague, Dr. H. Kennedy, during the debate which followed Dr. Little's paper, that digitalis should be given as a single remedy, in order efficiently to test its value. I endeavoured to carry out Dr. Kennedy's suggestion as far as possible. I believe the observations and cases which I am about to lay before the Society will confirm Dr. Little's opinions, and possibly add something thereto.

It may now be considered as an established fact that digitalis is a stimulant to the circulation, and Dr. Fothergill has suggested that it may be employed as such in the treatment of acute asthenic disease. By a stimulant to the circulation I wish to be understood as meaning a drug which has the property of maintaining that tone in the muscular fibres belonging to the heart and blood vessels, which is necessary to the maintenance of that degree of blood tension required for the efficient circulation of the blood.

Loss of tension is the great character of the pulse of acute adynamic disease, and this loss of tension is known by the frequent soft pulse, so particularly well-marked in typhus. Typhus is, therefore, the disease best suited for testing the value of digitalis as an agent for increasing the tension of the pulse. In the present paper I shall, therefore, confine my remarks to typhus, although I have employed digitalis as a vascular stimulant in other acute diseases, namely, pneumonia, enteric fever, and delirium tremens.

In the *Dublin Quarterly Journal of Medical Science* for February, 1867, was published a paper which I read before this Society during the session 1866-67, upon the pulse of typhus. I there pointed out the low tension characteristic of the typhus pulse during the height of the disease, and showed, I think, pretty clearly that this low tension depended upon two conditions, namely, great flaccidity of the capillary vessels and diminished power of the heart. I believed then and do so now, that the former of the two conditions is the cause of the low tension at the commencement of the disease, and that the latter condition is added after the disease has continued for some days.

We might, therefore, expect that a drug which either diminished the flaccidity of the capillaries by producing their contraction, or increased the contractile power of the heart, would improve the tension of the circulation, and therefore improve the character of the pulse. That digitalis can fulfil both these objects is the view now held by many of the highest authorities.

From the various experiments and observations by Brunton, Fothergill, Hanfield Jones, Dybkowsky, Pellikan, Hilton Fagg, Schiff, Valentin, Stevenson, Traube, Von Bezold, Blöbaum, and others, it is now definitely

settled that digitalis increases the power of the ventricular contraction. It has also been shown by several of those observers that this drug also diminishes the calibre of the capillary vessels. These observations have been lately confirmed by those of Ackerman and of Rostock, the former of whom seems (for I have not seen the original paper of the author, and am trusting to the abstract which recently appeared in the *Practitioner*, a journal whose accuracy I confidently rely upon) to have almost exhaustively investigated this subject. Most of the authors I have noticed agree that the increased power and diminished frequency of the ventricular contractions are due to the power which digitalis possesses of increasing the inhibitory action of the vagus, in the same way that electrical or other irritation increases that action. If this stimulation is carried to too great an extent, a reverse action follows (as seems to be usually the case in all kinds of excessive stimulation), and a state of paralysis sets in, which is shown by great slowness, and then increased frequency of the pulse, sometimes followed by sudden arrest of the ventricular action, and always by irregularity. This irregularity is referred in Ackerman's paper to the combination of large beats, caused by ventricular, and small beats, by auricular contraction. When this stage is reached it is stated that the heart never resumes its ordinary condition, and death is the result. I am glad to state this is not always the case, as shown by one, if not two, of the cases I have had under observation.

As to the cause of the contraction of the capillaries, there seems to be a difference of opinion between the authorities; Traube and Böhm believe it to depend on the action of the vaso-motor centre in the medulla oblongata; Ackerman to the direct action of the poison on the vessels, as proved by the experiments of Fothergill and others; Heidenhain believes there is increased cooling of the central parts of the body from increased rapidity of the circulation, bringing cooled blood from the surface; but according to Ackerman, this does not seem to explain the effect of digitalis in lowering fever temperatures, which he believes to be caused by an asthenic condition of the circulation in addition to the diminished frequency of the pulse. In the only case I have yet met with (Case II.) where digitalis seemed to produce dangerous symptoms, I believe the condition mentioned by Ackerman really arose; the pulse fell to 60 from 120, and the temperature from $101^{\circ}.4$ to 99° , accompanied with coldness and great depression, which continued for twenty-four hours, the former conditions being resumed on omission of the digitalis. It must, therefore, be a serious question whether sudden lowering of the pulse and temperature by the influence of digitalis is not a dangerous, instead of (as some believe) a beneficial practice. In the majority of my cases I found the temperature not lowered or materially altered from the usual course. Ackerman considers the use of digitalis in fever as of doubtful value, but of great value in certain heart affections, especially in the weak heart, as

denoted by Dr. Stokes. Of this I have an example in the present paper (Case VIII.) I think, however, from the way the evil effects of digitalis in fever are spoken of in the abstract of Dr. Ackerman's paper, from which I have quoted, that he was not writing of fevers as we understand the term, for he writes of digitalis as *causing* loss of appetite, nausea, and vomiting. How it could cause loss of appetite in a patient who may be said to have no appetite I cannot understand, and in my hands I have but once found vomiting occur, and in that case (Case I.) the amount of digitalis used was very small, too small, in my opinion, to have produced any effect.

If we compare together the low tension pulses of typhus at its height with the healthy pulse or the typhus pulse returning to its normal standard, the tension may be said to be lost, as shown in Figs. 1 and 2, where the

Fig. 1.



Fig. 2.



curve is a mere formless wave, or all but lost, as in Fig. 3, which is the

Fig. 3.



undulatory pulse of typhus as usually given in books. Now, it is evident that the test of the effect of a vascular stimulant in fever is the power that that stimulant has to improve the tension of the pulse, as shown by the sphygmographic tracing, or as felt by the physician of great experience. If we have a pulse of the form shown in Figs. 1, 2, or 3, and we can convert it into that shown in Fig. 4, or the healthy

Fig. 4.



form shown in Fig. 5, we improve our patient, and if we can prevent

Fig. 5.



the formation of a pulse such as that shown in Figs. 1 or 2, we prevent

the circulation of our patient falling into such a state of debility as to endanger his life.*

Fig. 6.



I trust that the following cases will satisfactorily illustrate the preceding remarks.

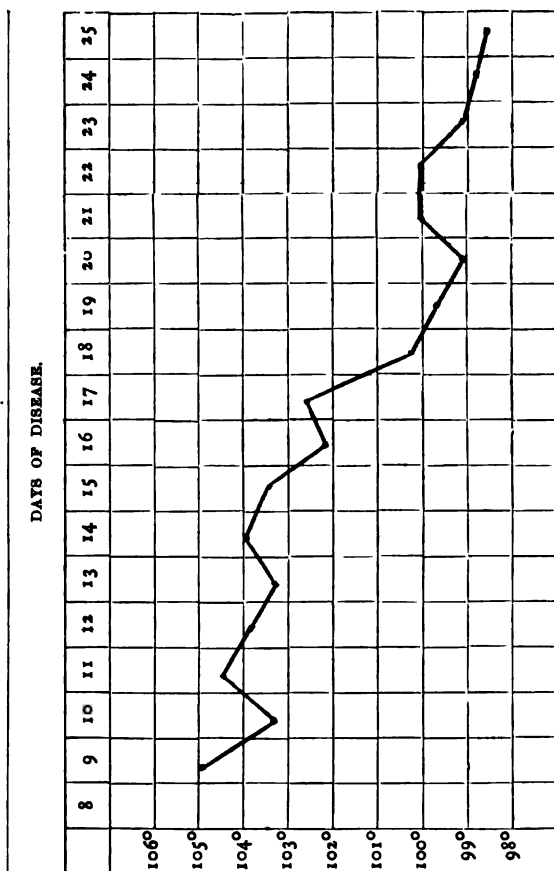
CASE I.—Anne Courtney, aged twenty-two, wardmaid, caught typhus in the discharge of her duty. First came under treatment on ninth day of illness, according to her own account, but I think she was about seven or eight days ill.

7th day (probably). Pulse 120, with fair tension, like White, No 9, Plate I.; temp. 105; heart sounds pretty distinct, but rather weak; tongue white with red tip, maculated densely. I meant this patient to get $\frac{3}{4}$ of infusion of digitalis every three hours, but owing to a mistake she got but $\frac{3}{4}$ in an $\frac{3}{8}$ mixture, so practically may be said to have had none until the next day, when she really did get the $\frac{3}{4}$ as directed, but immediately vomited. I do not think this vomiting can be attributed to the digitalis, as her stomach had been once sick before. I therefore tried to allay the irritability of the stomach, and postponed all other medication to the next (probably the ninth) day. On this day she was much weaker; pulse undulatory, like Mulally, No. 11, Plate II. I now gave her the digitalis as intended, which this time remained on her stomach. I continued to raise the dose daily until $\frac{3}{4}$ every three hours was reached. On the following day after this she got very cold, the pulse and heart became irregular, although the heart sounds were distinct. She seemed going into a state of collapse, but soon recovered by the aid of hot bottles and $\frac{3}{4}$ of whiskey made into punch. I continued the use of the digitalis on the following day without any evil effect. There was some bronchitis on the fifteenth and sixteenth days which did not become serious. She was convalescent on the twenty-ninth day, the pulse having resumed its tension on the eighteenth day, like Shelly, No. 12, Plate I., and the heart having a remarkably clear sound and strong impulse as convalescence took place.

Only three doses of whiskey of $\frac{3}{4}$ each were given during the whole course of this case; no wine was administered. There was some delirium between the seventh and eleventh days, but during this time the digitalis had not a fair chance. The pulse tension was never absolutely lost. The range of temperature (Fahr.) is given in Diagram I.

* For those unfamiliar with the sphygmograph I insert a tracing of the jerky pulse of a cerebro-spinal meningitis case, which closely resembles in feel that of the typhus pulse returning to its normal form under the influence of digitalis.

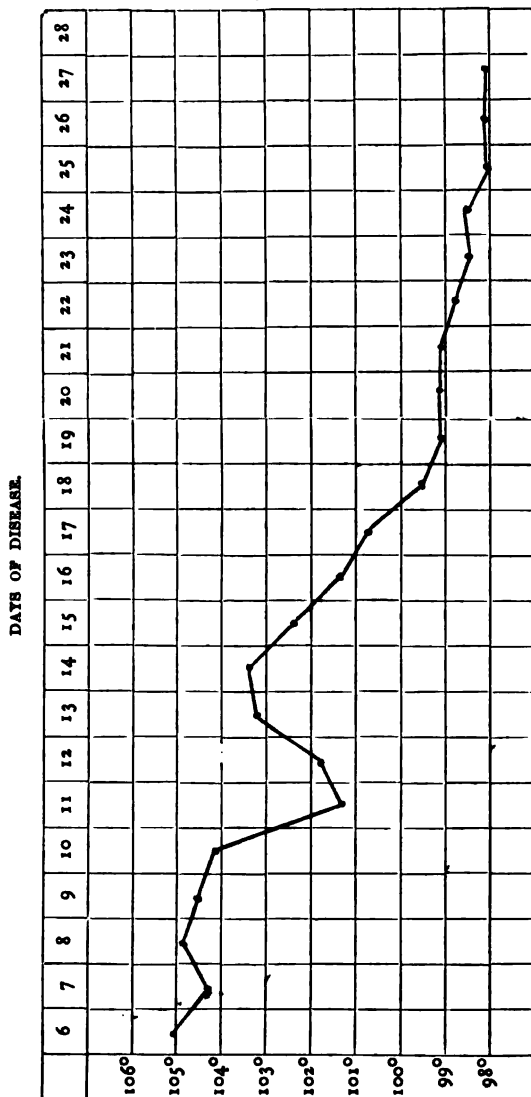
Diagram I.



CASE II.—Edward Drew, aged twenty-four years, a sailor. This patient came from Boston, U.S., to the port of Dublin, and was ill from a few days after he started until his arrival in Dublin on November 3rd, 1872. He was admitted into the hospital, on the day after his arrival, in a state of apparent collapse, which, however, proved to be a febrile rigour. He had had a previous rigour of this sort on the voyage, and had two more during his stay in hospital. His debility was extreme, so much so that he required for many days large quantities of wine and whiskey to maintain vitality. This stimulation reached the amount of 320 of wine and 38 (in one day 314) of whiskey daily. The patient was attacked with phlegmasia dolens of the right leg, which lasted for about ten days. He was convalescent from the 14th of December until the 8th of January, when he unfortunately, before he had recovered his full strength, was attacked by typhus fever.

I need hardly say a more unfavourable prospect could scarcely be afforded than that of a man just recovered from a long, tedious, and complicated illness being attacked by typhus. He became densely spotted, his heart sounds became weak, and his pulse tension very low. On the seventh day it was like Shelly, No. 6, Plate I.; on the eighth, like Shelly, No. 7, Plate I.; on ninth, nearly the same as on eighth. The range of temperature (Fahr.) is given below (Diagram II.), from which it will be

Diagram II.



seen that a high temperature was maintained for an unusually long time. Digitalis was used in this case at first in small $3\frac{1}{2}$ doses of the infusion, but ultimately in $\frac{3}{2}$ doses every third hour. The pulse tension was quickly restored, and the heart sounds gained strength.

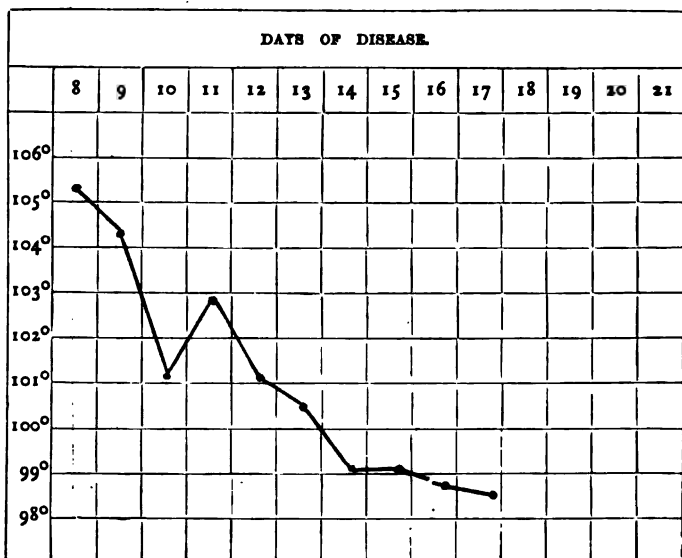
Knowing the great debility which occurred in this case in his previous illness, I did not like to rely altogether upon the digitalis, so gave him a little wine, at first $\frac{3}{4}$ of Marsala per day, which was increased to $\frac{3}{6}$, and on one day, and for that one only, he got $\frac{3}{10}$. The contrast is strong between the amount of wine and whiskey required in the first and in the second illness, the smaller quantity necessary being, I believe, altogether owing to the assistance given by the digitalis.

CASE III.—Margaret Byrne, aged nineteen, machinist; first seen on eighth day of disease.

Day eighth.—Densely maculated; heart sounds of a foetal character; tongue brown and moist; headache; bowels regular; temperature $105\cdot2$ Fahr.; pulse 136, of low tension, like Mulally, No. 8, Plate II. Ordered infusion of digitalis $\frac{3}{1}$ every third hour.

The range of temperature in this case is given below (Diagram III.) The pulse fell to 120 on the ninth day, although the tension was not improved. The pulse and temperature both rose on the tenth day, when bronchitis appeared. The tension of the pulse was restored on the twelfth day; like White, No. 9, Plate I., although it may be said to have

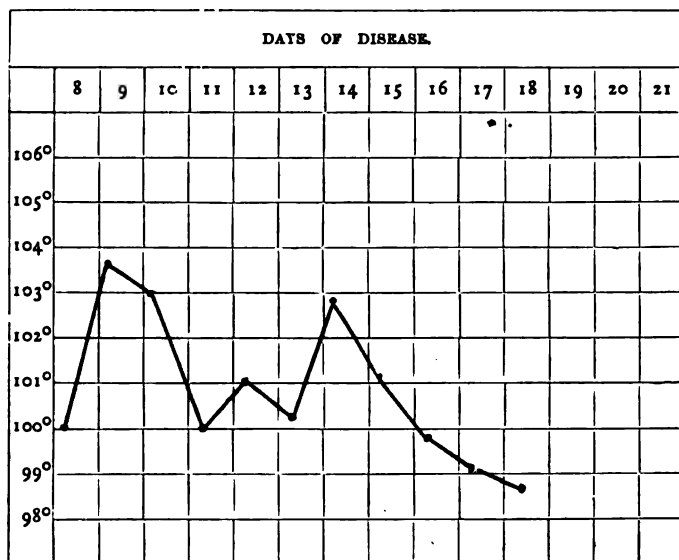
Diagram III.



been lost on the eleventh, like Fig. 2. The dose of digitalis was raised on the eleventh day to $31\frac{1}{2}$ of the infusion, and turpentine was given with it on account of the bronchitis. No alcoholic stimulant was given at any time. No delirium occurred. There was no dryness of tongue, and the temperature seems to have followed the usual course, the rise on the eleventh day being due no doubt to the increase of bronchitis. Completely convalescent on twenty-first day.

CASE IV.—John White, a labourer, aged nineteen years; first seen on eighth day of disease; is a heavy, fat soft man, given to habits of intoxication. Pulse only 80, and of character shown in No. 8, Plate I., giving the daily pulse tracings of this case, which exhibits rather low tension, also well developed dicrotism and rounded summit. Temperature $101^{\circ}\cdot 1$ Fahr. (Diagram IV.). Tongue light brown, centre, edge, and tip,

Diagram IV.



red; had pain in head before admission, which is now nearly gone. Sounds of heart distinct but weak; some doubtful red spots, and doubtful typhus mottling; had slept a little during previous night. Ordered diaphoretic mixture.

On ninth day symptoms nearly same; pulse quicker, as shown in No. 9, and spots well marked; ordered infusion of digitalis in 32 doses every third hour. On next day the pulse was the same as on the eighth day, but heart sounds much weaker, and spots darker but not very densely set on the skin. The digitalis was used to $3\frac{1}{2}$ doses and 34 of

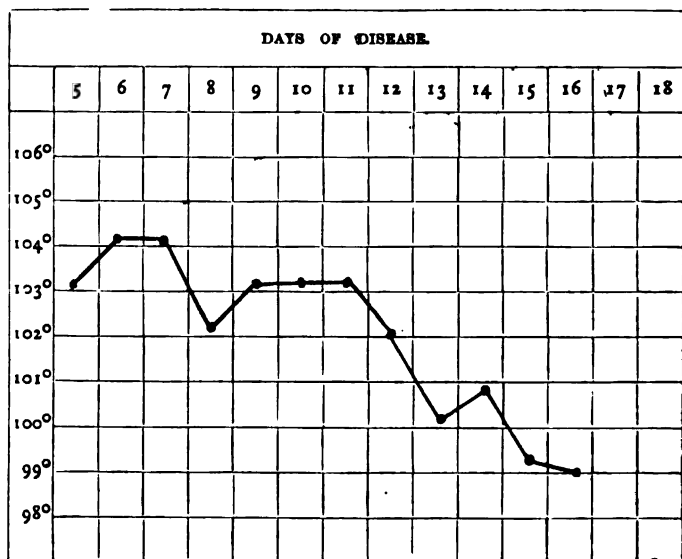
whiskey given. The case got a little worse the next, the tenth day, when the heart sounds were weaker and pulse not so distinct. The digitalis was increased to 36 doses. From this time the patient steadily improved and made a good recovery.

The pulse tracings in the case, as shown in Plate I., demonstrate that the tension never fell very low. There never was any delirium. The amount of whiskey necessary was extremely small, considering the habits, appearance, and symptoms of the patient.

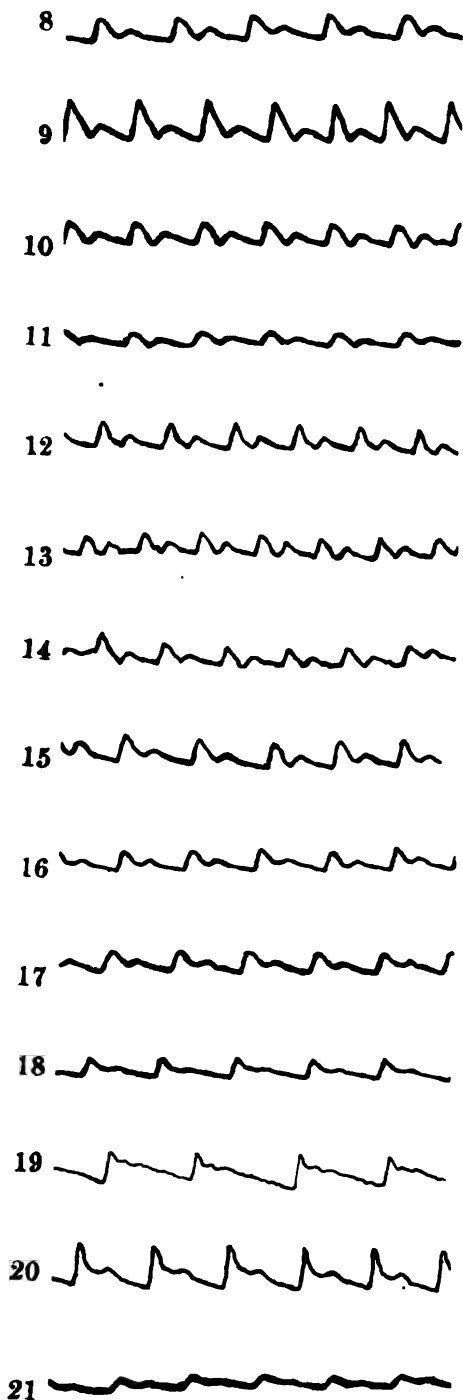
CASE V.—John Shelly, a shoemaker, aged twenty-nine, admitted on fourth day of disease. First seen on fifth day; a thin, dark complexioned man; admitted to "drinking a fair share." On fifth day the heart sounds were very weak; pulse 120; temperature (Fahr.) 103° (see Diagram V.). There was a little dulness over the lower part of the back at the left side; the tongue was whitish; some abdominal tenderness and flatus, but no local tenderness; bowels had been "loose," but not frequently moved, and not at all since admission; intense headache. Ordered turpentine stupe to abdomen, sulphuric acid mixture, and a leech to each temple.

Sixth day—Heart sounds stronger; head better; pulse 120; temperature 104°; eyes suffused; has a cough and some slight bronchitic sounds in chest, but says he has had a cold for a month. Ordered sulphuric acid to be discontinued; infusion of digitalis to be given in 3½ doses every third hour, and iodine liniment to be applied to chest.

Diagram V.



WHITE.



SHELLY.



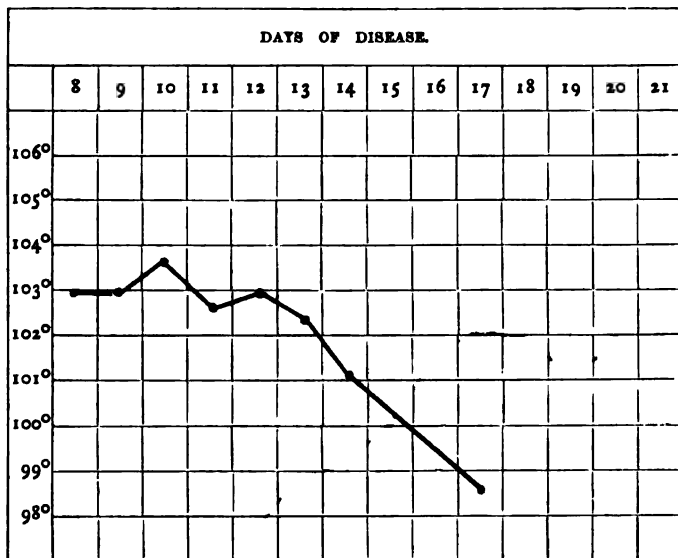
The pulse tracings in this case (see Plate I.) show that the tension of the pulse was very low at the beginning on sixth day, on which day digitalis was ordered. The pulse improved in character on the seventh, and then steadily improved until the pointed character of the summit of the tracing was attained on the thirteenth day, from which convalescence progressed quickly. No alcoholic stimulants were given. There was no delirium in this case. I believe here the pulse was maintained in comparative strength and improved by the digitalis.

CASE VI.—J. Healy, aged sixteen, a gasfitter. First seen on ninth day of illness.

Ninth day.—Tongue red at edges and tip, centre white, eyes very much suffused; maculated, especially on thighs, shivering, heart sounds good; said to have had hæmorrhage from bowels, but this seems doubtful, as there was none after admission; diarrhœa, dark in colour, but not in profuse discharges. Ordered dilute sulphuric acid 15 m. every third hour.

Tenth day.—Raved during night; passed urine and fæces under him without notice; tongue brown; pulse 104, of fair tension, like Mulally, No. 9, Plate II; temperature (Fahr.) 103°·4 (see Diagram VI.); heart sounds very weak. Infusion digitalis $\frac{3}{4}$ every third hour.

Diagram VI.



Eleventh day.—Headache severe; heart sounds improved; spots as yesterday; tongue moist and fur lighter; some bronchitis on right side; temperature 103°·6; pulse 120; tension improved; still passing under

him, but as there was some urine in the bladder a catheter was used, and a small quantity drawn off.

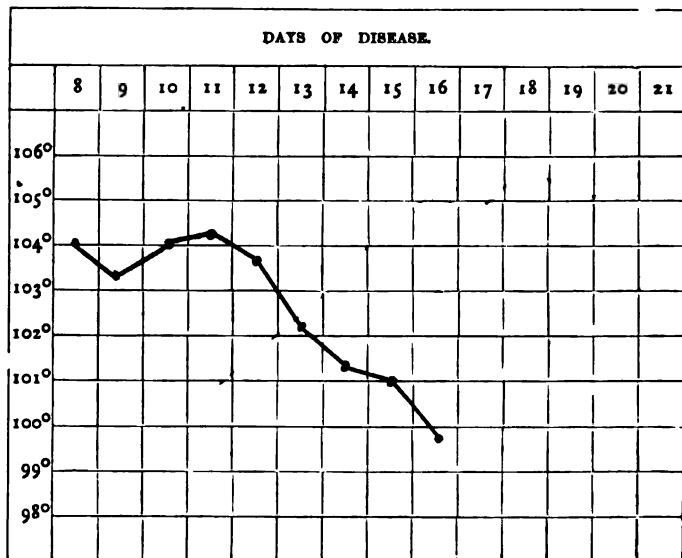
Twelfth day.—Sordes on teeth; tongue red; moaning all night; still passes under him; heart's action not so good; headache gone; temperature $102^{\circ}7$; pulse 103. Infusion of digitalis $\frac{36}{3}$ every third hour.

Thirteenth day.—Much same state, but spots fading. He was convalescent on the eighteenth day. No alcoholic stimulant was given.

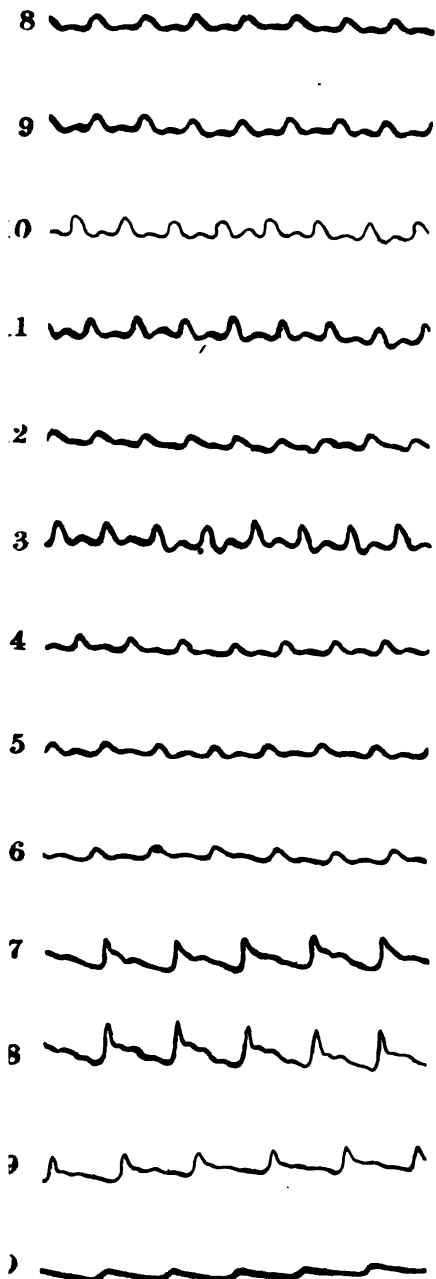
CASE VII.—John Mulally, aged twenty-eight. First seen on seventh day. The variations in the pulse of this case are seen in Plate II., and his temperature (Fahr.) in the diagram below (see Diagram VII.).

On the eighth day he was maculated; his tongue white; temperature 104° ; pulse 120, and of tension shown in No. 8, Plate II.; the heart sounds were weak; he was ordered $\frac{3}{4}$ of infusion of digitalis every third hour. He slept well on following night. His headache was so severe on the eleventh day that leeches were applied to the temples. On the same day he had some bronchitis, and the temperature rose. He was delirious on the night of the fourteenth day, but it was of a faint character, although he tried to get out of bed, but was easily restrained. He was convalescent on the seventeenth day. His heart sounds were restored on the eleventh day. After he had been up for a few days he was attacked with violent pain in the left ear, which returned several times during convalescence, and ultimately suppuration took place, and I am afraid the hearing is permanently damaged.

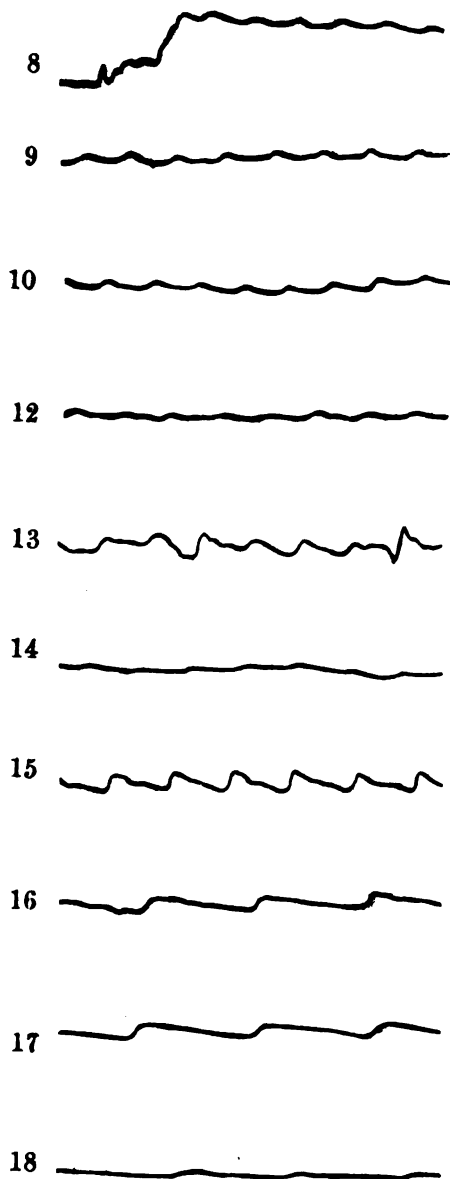
Diagram VII.

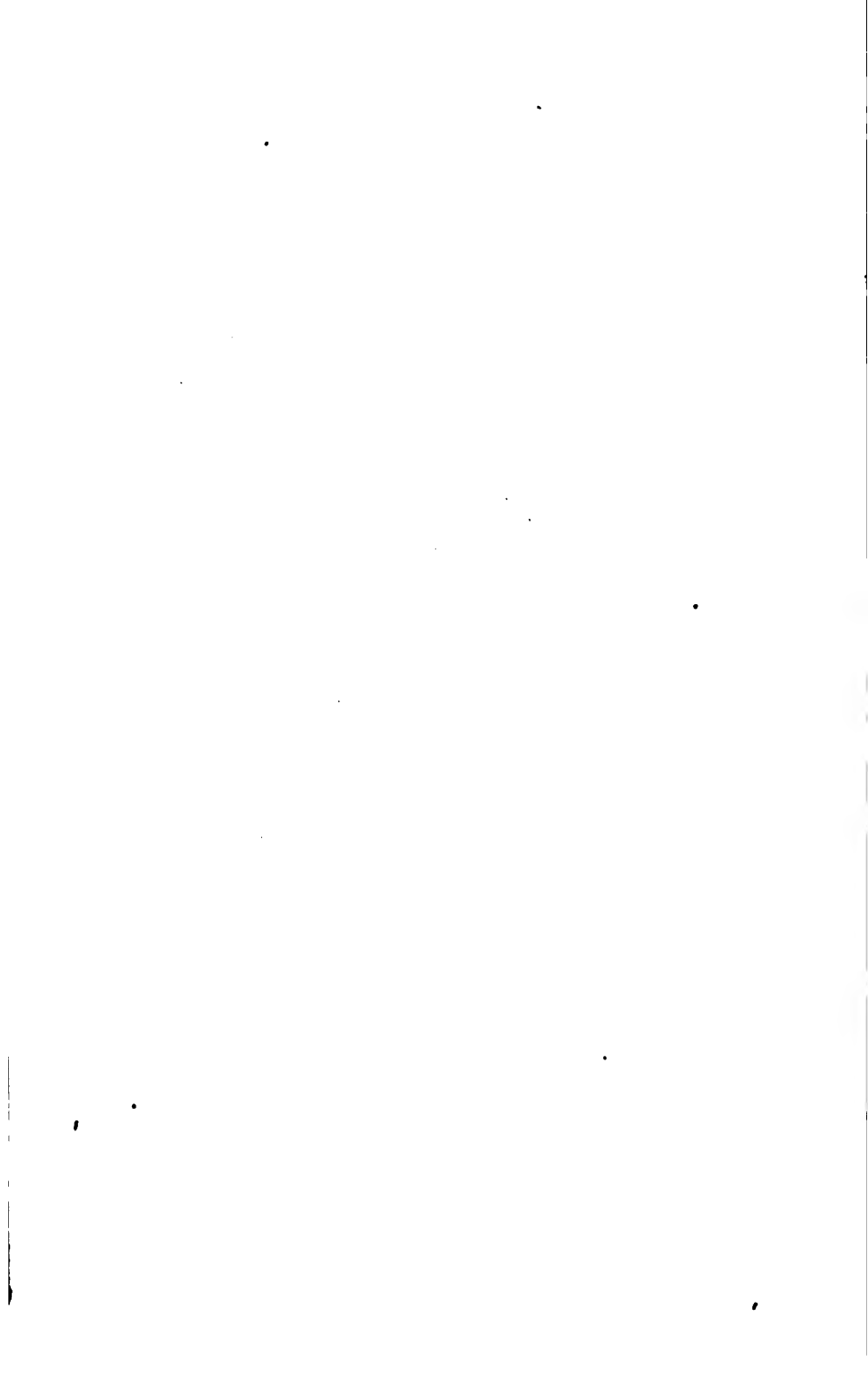


MULALLY.



MARY WALSH, aged 49.





This patient got no alcoholic stimulants; infusion of digitalis was given to the amount of $\frac{3}{4}$ every third hour from the tenth to the fifteenth day, when, as the pulse tracing had resumed its normal form, the dose was reduced to $\frac{3}{8}$. On the seventeenth day the tracing had the pointed character denoting sudden and quick contraction of the ventricle. The digitalis was discontinued when the pulse, though small, assumed the usual healthy form (No. 20, Plate II.).

It will be seen from the pulse tracings, in Plate II., from this case, that the tension had fallen very low, and the pulse became almost undulatory in its character on the tenth and eleventh days, but soon recovered under the continued use of digitalis, falling in tension only on the thirteenth day, when the bronchital complication arose.

CASE VIII.—Mary Walsh—the pulse tracings from this case are shown in plate II.—aged forty-nine, married, has had thirteen children, three of whom are in hospital, cases IX. and X., and a little girl with a mild febrile attack—is a fat soft woman, with arcus senilis around cornea. Admitted on sixth day of disease; first seen on seventh day, when temperature was $102^{\circ} \cdot 2$ Fahr. (see Diagram VIII.), and pulse 110. No tracing was taken, but the pulse was small and weak; the heart sounds were distinct, but first sound weak. The patient was in great anxiety about her son and daughter who were sick; the bowels were confined; tongue white, with red edges and tip; ordered castor oil $\frac{3}{4}$ and infusion of digitalis $\frac{3}{4}$ every third hour.

On eighth day.—Heart sounds weaker, the first sound being nearly absent; tongue brown; other symptoms nearly the same; maculated; ordered digitalis as before, and whiskey $\frac{3}{4}$ daily, in divided doses.

9th day.—Spots unusually large and dark, but not numerous; first sounds of heart lost; says she is going to die; slept well; first sound of heart absent; add $\frac{3}{4}$ liquor strychnis to digitalis, and give dose every second hour.

Tenth day.—Heart sounds improved; first sound audible; slept badly.

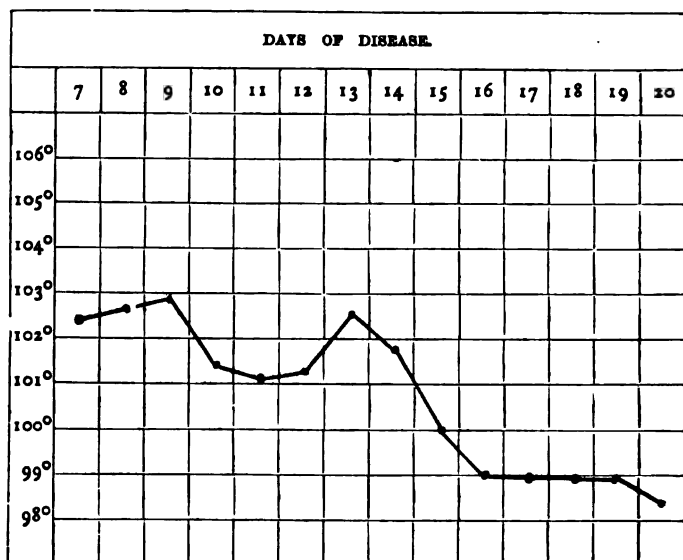
Eleventh day.—Slept a little; some subsultus; no tracing of pulse could be obtained; tongue brownish, but moist.

Twelfth day.—Heart sounds failed again, but recovered under temporary administration of $\frac{3}{4}$ of extra whiskey, and on the thirteenth day the heart sounds were again distinct. On the fifteenth day bronchitis appeared, which was preceded by a rise in temperature on the two previous days. The spots began to fade on same day, and the bronchitis soon became better, the patient being convalescent on the twentieth day.

On the thirteenth day the pulse tracing assumed the character which I am accustomed to associate with fatty heart, and this character was maintained during convalescence, namely, a tendency to squaring of the

summit of the trace, as shown in sphygmograms for the thirteenth, fifteenth, and sixteenth days.

Diagram VIII.

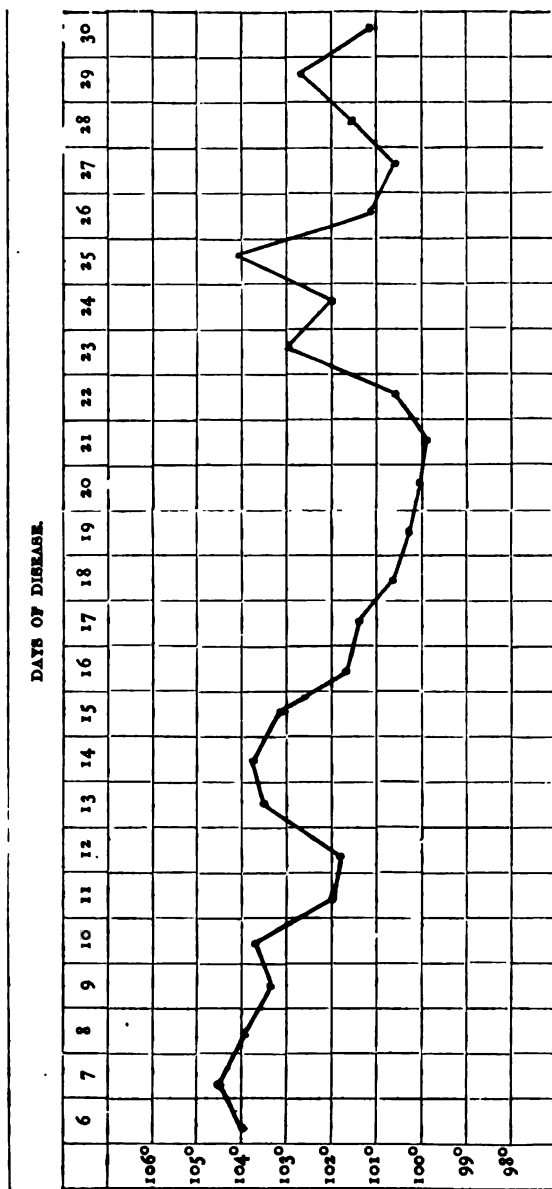


This I consider a very valuable illustration of the power of digitalis as a vascular stimulant. We had here a weak heart before fever commenced in a woman past middle age, who had borne a large family, and at the time of her illness subject to great mental anxiety, so great as to make her believe in impending death, a symptom which most physicians will, I think, agree with me in considering a very grave one. Such a case could scarcely have passed through such a serious illness in so comparatively a short time, without greater stimulation than was afforded by the whiskey given, never beyond 38 in twenty-four hours, and that for but one day; on two other days she got 36 of whiskey, and on others 34, commencing with this on the eighth day. She also had small doses of strychnia, a drug which I consider of great value in the treatment of weak heart, whether of fevers or chronic disease. I have used this drug in consequence of being impressed with its value as employed by my friend Dr. Henry Kennedy.

The temperature of this case never rose high, a condition which I attribute to the presence of fatty heart, not to the digitalis treatment. I have observed this tendency to a range of temperature lower than usual in other patients whose hearts were affected by fatty degeneration when such were attacked with acute febrile disease.

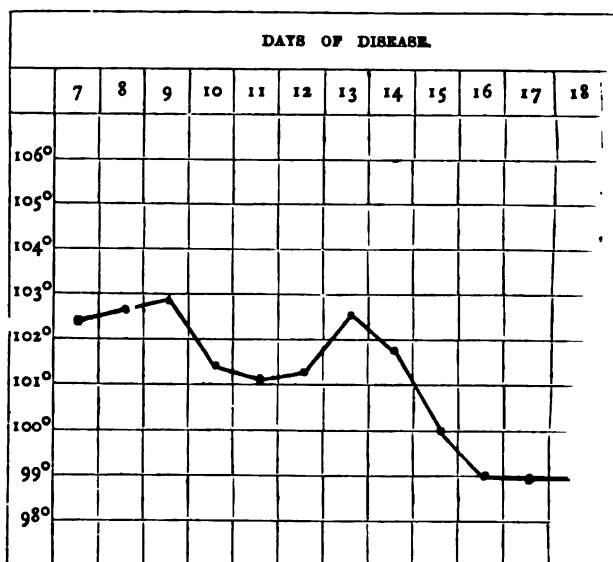
CASE IX.—Mary Walsh, aged twenty, a dressmaker. Mother has typhus (Case VIII.); brother has typhus (Case X.); younger sister has febricula. Has been a delicate girl, although she seems in pretty good

Diagram IX.



summit of the trace, as shown in sphygmograms for the fifteenth, and sixteenth days.

Diagram VIII.



This I consider a very valuable illustration of the power of a vascular stimulant. We had here a weak heart commenced in a woman past middle age, who had borne a child at the time of her illness subject to great mental anxiety, which made her believe in impending death, a symptom which, I think, agree with me in considering a very grave case could scarcely have passed through such a series of changes in a comparatively short time, without greater stimulation by the whiskey given, never beyond 38 in twenty-four hours for but one day; on two other days she got 36 of whiskey, 34, commencing with this on the eighth day. She also received a small dose of strychnia, a drug which I consider of great value in a weak heart, whether of fevers or chronic disease. I am in consequence of being impressed with its value by the experience of my friend Dr. Henry Kennedy.

The temperature of this case never rose high, a fact which I attribute to the presence of fatty heart, not to the disease. I have observed this tendency to a range of temperature in other patients whose hearts were affected by fatty degeneration. Such were attacked with acute febrile disease.

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DR. HAYDEN thought very few physicians had sufficient knowledge of the subject to render it probable that it would be discussed at a special meeting this session in a manner creditable to the Society. It would be far better to postpone the discussion to the next session. Dr. Grimshaw had given the matter a stimulus, and between this and next session it would probably receive such attention from the members of the Society, that they would be in a better position to discuss it then.

DR. AQUILLA SMITH agreed with Dr. Hayden, that very few physicians had directed their attention to the view put forward by Dr. Little, and he apprehended if they were to adjourn the present meeting, there was not sufficient experience among the members of the Society to enable them at a meeting to be held within a few weeks, to deduce an established rule as to the administration of digitalis in typhus fever. He had no doubt that in the interval between this and next session, Dr. Grimshaw would pursue the subject and bring it before the Society in a more practical way than on the present occasion. He did not hear much that night to satisfy him that any special or characteristic action was exerted by digitalis in the cases detailed. There was no particular group of fevers, and the distinctive characteristics of each case were not sufficiently specific to enable them to draw an inference. In many cases the digitalis was accompanied with a considerable amount of stimulants, six or eight ounces of spirits, which, no doubt exercised some influence. It was a subject very deserving of careful clinical observation.

The CHAIRMAN could bear out what Dr. Grimshaw had stated that at the present time the amount of fever was not such as to warrant any general conclusion. He was anxious himself, after hearing the valuable paper of Dr. Little, to try the effects of digitalis; but he had met with only one case that he thought would do justice to it as an experiment. It was a case of very severe maculated typhus in a woman of about forty-six years of age, who had an attempt at crisis by perspiration on the eleventh day of her fever. All the other cases of fever under his care for some time were of a mild type and all recovered. This one was particularly severe, and resisted all the ordinary forms of treatment, the symptoms being referable chiefly to lesion of circulation. About the sixteenth or eighteenth day he ordered large doses of tincture of digitalis, combining with each dose fifteen drops of the muriated tincture of iron, but without any success. She died on the twenty-second day. That was the only case which he had witnessed in which he thought it fair to digitalis to experiment with it.

DR. FOOT said he should like to be made more certain on one point—namely, whether digitalis was administered by Dr. Grimshaw *ab initio*,

before the character of the typhus was distinctly known, or whether it was given only in decidedly bad cases, where it became a matter of anxiety as to whether the patient could be pulled through. Proof of its good effect in very serious cases would be of great value, but he would be very doubtful about any inferences drawn from the treatment of indiscriminate cases of typhus fever *ab initio*, with any single medicine. It did not occur to him that any of the cases brought forward by Dr. Grimshaw were very bad cases, except those two that died, and the elderly woman with the fatty heart. The other cases did not appear to him to be seriously complicated in the lungs or heart. Dr. Grimshaw made some remarks as to the effect of digitalis on temperature. He (Dr. Foot) perceived, however, that according to the charts exhibited, the observations on temperature were made only once a day. He would not consider the records of temperature of great value if they were not taken twice at least every day, in the evening as well as in the morning. In some of Dr. Grimshaw's cases six ounces of whiskey were considered a small quantity. Dr. Foot considered this a rather large amount of whiskey for ordinary cases of typhus, and if that amount were given with digitalis, he should not know whether to attribute the benefit to the whiskey or to digitalis. Unless it could be shown that in difficult, critical, and dangerous cases, digitalis used alone produced a good effect, he should not feel that it was a trustworthy medicine to employ in typhus.

DR. HENRY KENNEDY had no experience in the matter brought before the meeting by Dr. Grimshaw. There was one point, however, on which he wished to make a remark—in reference to the cases detailed, where symptoms of sinking and collapse seemed to follow the use of the digitalis. He thought that such cases could be explained differently, for such an occurrence was by no means uncommon in some cases of fever where no digitalis had been used. It was more frequent, too, in some years than in others; and crisis often showed itself in this very way.

DR. HAWTREY BENSON had used digitalis in two cases of malignant typhus. In one case the first sound of the heart was inaudible for several days. The sound had already become inaudible when he commenced the treatment, and the subsequent return of the first sound was much more rapid than he had usually experienced before. While the first sound still continued quite inaudible the impulse of the heart became restored to its normal strength.

He considered the effect of the digitalis must be carefully watched, for, in the case he referred to, some days after the patient had begun to improve, a sudden depression of the pulse to 48, and of the temperature to below the normal, and a great intermission of the heart's action, caused considerable alarm for several days. But for this he took the

blame entirely to himself, as he should have stopped the administration of the digitalis at an earlier date.

He thought the drug was capable of being turned to good account in these cases, and that if the matter were studied much might be done by it.

DR. C. F. MOORE mentioned the case of a young lad in whom the heart sounds went down to 46 in a minute, and the boy made a very good recovery. No digitalis was given.

DR. GRIMSHAW, in reply, reminded some of the speakers that his observations were applied to the treatment of a particular condition—the weak heart in typhus fever. The weak heart was shown by the pulse tracings, and those tracings exhibited that evening were sufficient to prove what he asserted, that they were all severe cases of typhus. He had many other typhus cases under his care in which he did not use digitalis. The cases in which he used it were all cases that he knew from experience were likely to prove serious; but as to waiting until a patient got into a dangerous condition, as suggested by one speaker, before trying digitalis, he thought that would be a very bad practice, and unfair both to the patient and the drug. He thought, again, to refer to one case, that a fat, soft young man, in the habit of drinking, was a very bad subject for typhus; and if Dr. Foot looked at the pulse tracing of the case to which he referred, he would admit that it was a bad case. His (Dr. Grimshaw's) test was the nature of the pulse, and he distinctly stated that, judging by that test, he believed digitalis maintained the power of the heart. He did not treat every patient indiscriminately with digitalis because they had typhus fever. The cases he had treated with that drug were all well-marked cases of typhus; and although the number of cases at present prevailing was unusually small, yet the type of the disease was excessively severe. Dr. Foot thought he had taken the temperature only once a day, but, as a matter of fact, he had had it taken frequently—as often as he deemed necessary—even to once every two hours for 24 hours; and it was a frequent practice in the hospital to take records of the temperature twice or thrice a day. The reason he gave a daily temperature range in these cases was because he thought a daily temperature was of greater value for conveying general information to his audience than a number of temperatures taken at various intervals. Daily variations in temperature depended greatly on circumstances. A patient who lay awake all night would have his temperature higher in the morning than in the evening, while if he spent a quiet night he would have it lower. He (Dr. Grimshaw) had taken probably not less than 20,000 of these observations; and when first testing the question of temperature variations in disease, he visited

the hospital twice a day to see how matters were going on, but he now relied on the observation of his assistants. He was perfectly aware of the amount of value to be attached to the observations he had made, and he thought that a daily temperature chart would give a person who never saw the case a better idea of its real course than a greater variety of observations. As to the doses of digitalis, he gave doses every two or three hours; the patients got it in the night time if they were awake, and if it lowered their temperature, it would not have risen in the morning, as the patients were not allowed to pass from under the influence of the drug. Certainly, if he thought a drug were doing good he should never venture to discontinue it in order to see if the patient would get worse; neither should he wait to use it until the patient was dying. In one case he did not use digitalis until the fourteenth day of the disease, simply because he did not get the chance. He gave it to the patients because they were bad cases of typhus, and because they had a pulse that had the appearance of getting very weak. It is impossible to convey to another by description the nature of a pulse as estimated by the finger, but you can do so by means of a tracing. He would also state that he did not think eight ounces of whiskey, without any other stimulants, a very large amount of stimulation in a case of typhus fever. He had treated hundreds of cases without wine, whiskey, or other stimulant. He treated his patients and not their diseases. Some got a diaphoretic mixture, and got no wine, whiskey, or digitalis, and recovered without the use of any stimulants whatever. With regard to what Dr. Kennedy said as to the collapse that arises in the crisis of fever, he could not say positively that the collapse in the particular case which he had stated was due to digitalis. It looked like it, and raised his suspicions, and he thought it was highly probable that he had overdosed his patient. He did not say these cases positively proved anything, but he thought they were suggestive of the value of digitalis in the weak heart of typhus.

The Society then adjourned.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIFTH ANNUAL SESSION.

JOHN R. KIRKPATRICK, M.B., Honorary Secretary.

DR. ATTHILL, Vice-President, in the Chair.

Saturday, May 10th, 1873.

Epithelioma of the Uterus.

DR. KIDD said the specimen which he now brought under their notice was not, perhaps, of any great interest, from its being rare; but he thought that it was of importance that specimens of the kind should be laid before the Society. It was an example of epithelioma which he removed from the uterus of a woman on the previous day, in the Coombe Hospital. It grew from the right corner and posterior lip of the uterus; the anterior lip was very little affected, but the disease had begun to manifest itself there also. The mass which he exhibited was a very characteristic specimen of the cauliflower excrescence, or epithelioma of the uterus. The posterior lip was removed along with the tumour, and they had also before them the anterior lip, which was free from the mass of the tumour, but had more or less of that granular appearance, showing the commencement of the disease. The specimen was an interesting one in connexion with the question of operation for epithelioma. Epithelioma was a form of malignant disease which afforded the best opportunity for obtaining relief by operation. In fact, it was, he might say, the only form of malignant disease of the uterus in which an operation was fairly feasible, for it was so little malignant that some writers questioned whether it should be placed in this category at all; but they were all now pretty well agreed in looking on it as cancer, but less likely to contaminate the surrounding tissues than other forms of the disease, therefore they had a better prospect of giving relief by operation in these cases than in other kinds of cancerous tumours of the uterus. This woman was being run down by hæmorrhage. She had had profuse hæmorrhage going on for many months, and during the intervals of the hæmorrhage had a large quantity of fœtid serous discharge, which was exhausting her strength. The disease was very far advanced when the woman came under his observation, so that he held out no hopes of curing the disease; but he believed that by removing the tumour he had

checked the hæmorrhage for a time, and the serous discharge, which was as exhausting as the hæmorrhage, would also be checked for a time, and the woman's suffering would be lessened, and, he hoped, her life prolonged. It was fully explained to the woman before the operation was undergone, that it was one simply for relief, and not to effect a cure; but in some cases the hope of a cure might be fairly presented to the patient. Some few years ago he exhibited to the Society an epithelioma he removed from the uterus of a French lady, and he had a letter from that lady a short time ago in which she stated that she had remained since the operation in perfect health. On the 1st of June, 1872, he removed from the uterus of a lady a mass of epithelioma considerably larger than the specimen before the Society. In that case after the lapse of four months the disease returned. There was a new outgrowth, which was removed in the Rotunda Hospital. At the time he saw it the growth was not larger than a hazel-nut. He was told a short time ago that this lady was pregnant, and in the enjoyment of comparatively good health. It was of importance they should know that in cases of epithelioma an operation will often effect a cure, and save life; and even if it does not save life, it will lessen suffering, and prolong life. One case has been recorded where, at the expiration of thirteen years, the disease had not returned.

Ovarian Tumour.

The other specimen which he had to bring under their notice was an ovarian tumour, which had been removed from an unmarried woman, twenty-eight years of age. She first noticed the tumour in May, 1871, in the left hypogastric region. It was painful from the first. She came under his observation soon afterwards. She was very anxious to have the tumour removed, but for a considerable time he refused to operate, as the tumour was not interfering with her general health, or of such a size as to require operation. In February of the present year the tumour was much increased in size. She measured at the umbilicus thirty-four inches, at the crest of the ilium thirty-five and a-half inches, from the ensiform cartilage to the umbilicus six and a-half inches, and from the ensiform cartilage to the pubis fifteen inches. There was a general crepitus over the right side. On the 20th of February he removed the tumour. The cyst was nearly unilocular, but there was a portion of solid at the base. After removal the cyst weighed 1 lb. 6 ozs. The fluid it had contained 160 ozs. (measured). It was perfectly free from adhesions, and as soon as the cavity of the abdomen was opened it almost shot out on making a little pressure on the abdominal walls. The woman recovered without a symptom of inconvenience, and was now in the enjoyment of good health. The only feature that retarded her recovery was that some weeks after the operation, and when nearly ready to leave the

hospital, she got an attack of local peritonitis. She complained of pain in her right side, and a tumour formed there, but this had almost completely subsided, and she was able to go about, and was in the enjoyment of good health. Before operation her menstruation was very profuse, but after operation her menstruation was healthy, and not in excess. During the second menstruation some menstrual fluid exuded through the cicatrix, an occurrence frequently noted by Mr. Spencer Wells.

DR. JOHNSTON said the Society should be thankful to Dr. Kidd for bringing these cases forward. One of the cases he had mentioned—that of the woman who was now pregnant—had come under his observation. It was some months since he had removed the epithelial growth, and when he saw the patient last, about a month ago, there was no trace of the cervix, and no outgrowth at all. The woman was now pregnant, and the uterus remained healthy. He had in the Rotunda Hospital three months ago a case similar to that which Dr. Kidd had brought forward. The woman came from the country suffering intense pain and wasted by profuse hæmorrhage. There was a large epithelial growth from the anterior wall of the cervix, which he removed with the scissors. He wished to bring forward that case to show there was not that danger in using the scissors which some supposed, and in this case it was adopted because he could not use the *écraseur*. There was not a particle of hæmorrhage, nor had there been a single drop since. The patient left the hospital five weeks ago. She was asked to come back if there was any trace of hæmorrhage, and as she had not done so he took it for granted there was none.

DR. RINGLAND said Dr. Kidd's case presented two aspects of very considerable importance—one, how far epithelioma of the neck of the uterus was curable by operation; and next, how far treatment could be followed in a palliative view. Dr. Kidd showed that the operation was perfectly justifiable when there was any sound structure to proceed upon. He could bear testimony to the accuracy of the views he had put forward on that point. Even as palliative treatment they were equally justified in employing the operation, even though the case might seem to be hopeless. He ventured to say this because of some observations that had fallen from some of their English brethren at the meeting of the British Medical Association in Birmingham, condemning a medical man for attending on cases of this kind when they seemed to be hopeless. He ventured on that occasion to put forward the opinion—and the majority agreed with him—that it was their bounden duty, as long as there was the least hope of relieving or palliating the distressing symptoms that presented themselves in such cases, to do what they could, even though they could not prolong life, to give comfort and ease to the

patient, no matter for how short a time. The cases submitted by Dr. Kidd proved the correctness of this view. In the particular case brought before the Society the disease had progressed too far to give any hope of a cure. He believed the body of the uterus was very deeply engaged; the posterior neck was engaged in the disease as far as the finger could reach, and he believed the body of the uterus was completely diseased. Still there could be no question the removal of this great mass, and the consequent cessation of the discharge from that growth, was of great benefit to the patient, relieved her distressing symptoms, and would probably suspend for some time the progress of the disease, for by means of the actual cautery the diseased structure might be destroyed. He believed it was their bounden duty, therefore, not merely to employ treatment with a curative view, but to employ it with the palliative view which Dr. Kidd had pointed out.

DR. CHURCHILL perfectly agreed with Dr. Kidd and Dr. Johnston that when a large mass of epithelial growth could be removed it was right to do so for the benefit of the patient. He could not say he was so hopeful of cure in these cases as Dr. Kidd. He had a case in which he removed a large mass from a portion of the cervix, and left a perfectly healthy cervix, three years ago. He saw her six months ago, and there was a bud coming from the old growth, and he heard since that the disease had developed itself in other portions of the uterus, and that there was a large tumour. The probability of cure, therefore, in these cases was not great; but that life could be prolonged by operation he had no doubt. That lady had enjoyed two years of a comparatively easy life, which was something. When once the growth was levelled by operation they had the means of keeping it down for a considerable time by the use of nitric acid.

The VICE-PRESIDENT (Dr. Atthill) agreed with Dr. Kidd and the other members who had spoken as to the value of this operation. It was his misfortune to have seen a considerable number of these cases. He had operated in several; the operation was painless to the patient and perfectly safe. In several of the cases which he had seen the disease recurred in a short time; in the most unfavourable an interval of two months elapsed before the patient was as bad as ever. His success in dealing with these cases had not been so great as Dr. Kidd's. As to the other question, whether they ought to operate, he was of opinion that they should. It should be borne in mind that some growths supposed to be malignant proved on subsequent microscopic examination not to be so, and they could not tell until after operation whether this were so. In such cases the removal of the growth would certainly result in a cure. He had very little faith in the application of any

form of escharotic. In several cases he had recently tried the Vienna paste, potassa fusa, and nitric acid, and in none had he succeeded in keeping down the reproduction of the growth.

Occlusion of the Vulva, cured by Operation.

DR. MORGAN exhibited a cast illustrating the cure of occlusion of the vulva by operation. The patient was a young girl, twenty-two years of age, very healthy and strong, who had been sent up from the country. The principal symptom she complained of was epileptiform seizures at each menstrual period. These seizures were so serious that the medical man in attendance upon her was very anxious about them, and sent her up to him. The following were the appearances, as represented in the cast on the table:—She was very well nourished. The external parts of the organ of generation were highly developed, but there was no appearance of an external vulva whatever; the parts were occluded by a cicatricial tissue forming a web from one side to the other. There was an aperture through which a small-sized catheter could be passed, which was the only one for the exit of urine and menstrual fluid. At certain times he could pass an instrument into the bladder, and at other times into some deficiency which was under this cicatricial web. The girl suffered considerable pain during the menstrual period, and also from the epileptiform seizures. He thought the account she gave was the correct one. She stated that seven years previously she suffered from fever, and the parts had sloughed, just as in the case brought before the Society by Dr. Byrne in the session of 1871, and when the girl recovered after the fever she found this web had formed across. Dr. Morgan proceeded to describe the operation. He passed a bistoury underneath and found the parts were free. He then split down and excised a part of the cicatricial web, and used a tent to keep it open. Underneath all the parts were perfect. There was not much difficulty in healing afterwards; it healed by retraction of the labia on either side, and the patient went on very well, and the epileptiform seizures ceased. After she went to the country she was examined and found to be all right. She had since been lost sight of. The woman whose case Dr. Byrne had brought forward afterwards married.

The VICE-PRESIDENT said the case was one of great interest, especially as regarded the convulsive affection to which the girl was subject, and which he believed was due to some reflex action.

Cases of Amenorrhœa from Congenital Malformation. By FLEETWOOD CHURCHILL, M.D., M.R.I.A., ex-President of the King and Queen's College of Physicians and of the Obstetrical Society of Dublin; Hon. Member of the Edinburgh, London, Berlin, and Newcastle Obstetrical Societies, and of the Gynæcological Society of America, &c., &c.

SIR,—No one can have been long in practice without meeting one or more cases of amenorrhœa from congenital malformation, and every one must have felt the difficulty of arriving at a satisfactory decision in such cases. This must be my apology for inflicting upon you the relation of some cases of this kind which have come under my observation.

It is clear that the question of relief to be afforded or not, must depend upon the accuracy of our diagnosis; yet all will admit, I think, that this is by no means easy—in some cases it is very difficult, and in others we can do little more than balance probabilities.

The means of forming a diagnosis are either physiological or physical, *i.e.*, combining the light afforded by a comparison of the physiological condition of the organs and the results of a minute and careful examination. Let me say a few words upon each.

1. As all know, the usual signs of puberty are menstruation, the development of the breasts and the external genitals.

But suppose that menstruation has never taken place, and that no effort—the menstrual molimen, as it has been called—has made itself felt, what is the precise value of this in forming our diagnosis? It may be either that the ovaries are absent or that they are inactive, or possibly that there is some impediment to the transmission of their influence, *e.g.*, impervious Fallopian tubes.

Again, in married women we have a further proof of ovarian action in the development of sexual desire and its gratification. Of course it is always a delicate, sometimes a difficult matter to ascertain this correctly, and not less as to appreciate its exact value. When present, I think that there can be no doubt of the presence of at least one ovary, and of its being more or less active, even though menstruation do not take place.

But the converse is certainly not true, the absence of sexual desire or gratification is no proof of the absence of the ovaries, unless other circumstances also lead to this conclusion. I have known many cases of women having large families whose entire life has been marked by the absence of these sexual characteristics. Whilst, therefore, positive evidence of this kind seems conclusive, negative evidence is of doubtful value.

2. In very many cases a careful examination will clear up our

difficulties; in others, it may increase the probabilities one way or the other, and in a few it may not assist us at all.

The examination should be very minutely made. Inspection will at once determine the adequate or inadequate development of the external organs. The finger will detect if the vaginal orifice be of the usual size, or if it can detect none, then an inspection must be made, and an exploration by a probe of the entire surface exposed by the complete separation of the labia. In a case I saw the other day, the finger failed to detect the orifice, but I found a very minute opening, which at first I thought might be the urethra, but as no urine followed the introduction of the catheter I sought and found the urethra, and it was then clear that the equally small opening was the vagina.

If the vagina be pervious and of the usual length, we shall be able to ascertain the presence or absence of the uterus, and by the careful and gentle use of the sound to measure its length and capacity.

But if the vagina be only an inch or two long, terminating in a *cul de sac*, we shall not derive much help from the examination.

Suppose, however, that we have ascertained that there is no vaginal orifice at all, the next point to be determined is, whether the vagina is closed by the apposition of its two sides or absent altogether. If we pass a silver catheter or sound into the bladder and one finger into the rectum, we shall be able to form a pretty correct opinion on this point by the thickness of the septum between the finger and the catheter; and if the catheter be passed to the further end of the bladder, and the finger as far as possible into the rectum, we cannot but feel the uterus if there be one, even though imperfectly developed. I think by this mode of examination we may arrive at a positive conclusion as to the presence or absence of vagina or uterus. In some rare cases one or both ovaries may be detected, but a negative result is of no value. Further, if the patient be placed on her back and we press down with one hand behind the symphysis pubis, placing at the same time a finger in the rectum, we can ascertain pretty accurately what there may be interposed.

I will now proceed to relate shortly a few cases which will illustrate these remarks.

CASE I.—Last year I was consulted by a lady from one of the colonies, aged 28. She was very tall and generally well made, but spare. She had been married several years, had no children, and had never menstruated. She had never had the slightest menstrual molimen, and had neither sexual desire nor gratification. The external genitals were normal, the vagina natural, the os uteri pervious, and the uterus only a little below the natural size. The breasts were undeveloped.

In this case, judging from the amenorrhœa, the defective breasts, and the absence of sexual instincts at her age, one cannot doubt that it was a

case of absent ovaries, or if not entirely absent, so atrophied as never to have exerted that influence which is the characteristic of the "primary formative organ."

CASE II.—Miss A. B., aged 22, full made, but not in good health. Has never menstruated, nor had any symptoms of its approach, nor leucorrhœa.

The breasts were pretty well developed, and other signs of puberty were present. The external parts were normal and the vagina natural, but the uterus much undersized. No ovaries could be felt.

In this case I inferred from the breasts and other marks of puberty, that there were ovaries, but probably as undeveloped as the uterus. I may add that the usual emmenagogues had no effect whatever.

CASE III.—Some years ago I was consulted about a young lady, aged 18, in whom no signs of puberty had appeared, neither menstruation nor mammary development. I was not permitted to make any examination, and therefore can only conjecture that the ovaries are absent, as I believe the young lady remains in the same physiological condition still.

In the first of these cases, I have little doubt that the ovaries were absent; in the second, notwithstanding the girl's age, that they were not acting. In the next, which I saw a few weeks ago, the defect is still more obscure.

CASE IV.—Mrs. D., aged 35, married six years; no children. Has never menstruated at all, nor had any periodical white discharge, nor any menstrual molimen. I found the external parts fully developed, the vagina normal, and the uterus in its natural situation; but the sound passed in barely two inches. No ovaries could be felt, but she told me that she had strong sexual desire and gratification. Here one could hardly doubt that there were ovaries, and certainly a uterus, but why did not menstruation take place? I cannot say, but I should rather conjecture that there is some obstruction in the Fallopian tubes, or in the continuity of the nervous influence.

Now, let us pass to another class of cases about which more certain information can be obtained.

CASE V.—Miss C. D., aged 20, a middle-sized, plump, well made girl, who has never menstruated, nor had any of the premonitory symptoms. The breasts are fairly developed, and other signs of puberty were present. The external parts were of the ordinary appearance and development, but when the labia were separated the clitoris and urethral orifice were visible, but the orifice of the vagina was closed. In order to decide as to the presence or absence of the uterus, a silver catheter was passed into

the bladder and a finger into the rectum. When the finger was pressed against the anterior wall of the rectum the catheter could be felt up to the fundus of the bladder and between the finger and the catheter, nothing but the septum between the rectum and bladder, which scarcely felt thicker than one vaginal wall. No uterus could be felt; but Dr. Kidd, who saw the case with me, thought he felt an ovary. Dr. McClintock also saw the case, and we all agreed that there was no uterus, but that the physiological evidence was in favour of there being ovaries.

Circumstances made it of great importance that the diagnosis should be placed beyond doubt, and therefore the young lady was taken to London and there consulted Dr. Priestley and Mr. Spencer Wells. The latter gentleman states:—"We gave methylene yesterday, and with a sound in the bladder and a finger in the rectum made a careful dissection or division of the raphè, or fourchette, and found a vagina, but it ends in a *cul de sac*. There is no uterus. Both ovaries can be distinctly felt in their normal situation. The fold of broad ligament can be felt running across the pelvis, but nothing like even a rudimentary uterus."

So far this was satisfactory, as confirming the previous diagnosis of the presence of the ovaries.

The wound was allowed to close, and the young lady is now as she was before the exploration.

CASE VI.—For permission to make use of the two following cases, I am indebted to Dr. Johnston, Master of the Lying-in Hospital; the notes have been kindly furnished to me by Dr. Cranny, Assistant Master:—

"Mrs. S. R., aged 26, married six years, but has never menstruated. The breasts are well developed, and the external organs normal. Internally the finger passed through the vaginal orifice for about an inch, and there the canal ended in a *cul de sac*. A catheter in the bladder could be felt by the finger in the rectum, with no uterus intervening. One hand passed down over the pubis could be felt by the finger in the rectum.

"She states that she experiences sexual excitement during the attempt at coitus."

There can be little doubt, I think, of the presence of the ovaries in this case.

CASE VII.—E. D., aged 19; unmarried. "Has never menstruated; complains of occasional headache, but gives no history of menstrual molimen. Breasts fairly developed, but with small gland. No hair on pubis or vulva; external organs like a girl of ten or twelve years. In attempting to examine per vaginam, the finger passes in about an inch and is then arrested in a *cul de sac*. A sound passed into the bladder

can be felt by the finger in the rectum, with a thin septum only intervening, and no uterus could be discovered. One hand passing down behind the pubis could be felt by the finger in the rectum. The pelvis is very narrow, measuring but $8\frac{1}{2}$ inches from one spinous process of the ilium to the other.

“Two glandular tumours, one at each side, below the inguinal ring and in the inguinal canal apparently, and the left one can be reduced inside the ring. Coughing gives them a slight impulse. They are about the size of ovaries, with a glandular feel.”

It is much more difficult in this case to arrive at a satisfactory conclusion. I saw the patient, and am satisfied that there is no uterus. The mammary gland to me felt very small, and the genitals resembled those of a child of 10 or 12 years of age. But what were the glandular bodies in the inguinal canal? They were about the size of ovaries, and had a glandular feel, and one could be returned within the ring. If they were ovaries they were absolutely inactive, or there would have been other signs of puberty. On the whole, I am inclined to think that they were not ovaries.

CASE VIII.—Some years ago the late Dr. O’Ferrall asked me to see a single woman, aged 28, in St. Vincent’s Hospital. She wished to marry, but conscious that she was not all right, she consulted Dr. O’F., at the same time stating that she had sexual desire.

She had never menstruated. The external parts were natural and fully developed, but the vagina ended in a *cul de sac* about an inch and a-half from the orifice. No uterus could be found. The mammæ were well developed.

The conclusion to which Dr. O’Ferrall came was, that although the uterus was absent, the ovaries were present, and I believe he was right.

CASE IX.—Some years ago my friend Dr. Cruise published in a very interesting paper an account of the dissection of a case of this kind, of which he says:—“The external organs were perfect, except the closure of the vaginal orifice. The internal organs were disposed as follows: the ovaries present and fully developed; the Fallopian tubes present, but in a rudimentary condition; the uterus represented by the coalescence of these organs; the round ligaments absent; the vagina absent.” After a very careful and learned disquisition, the Doctor draws the following conclusions:—“That the case under consideration is an example of development arrested at a certain recognized point.

“That the arrest took place before the completion of the third month of intra-uterine existence.

“That the Wolffian body was the blighted organ, and most especially its true excretory duct.

"That the case illustrates and confirms the observations hitherto made relative to the growth of the genital apparatus; and

"That it offers confirmation of the physiological law, that the sexual characters of the individual depend on the presence of the mammary formative organ."

In Dr. Cruise's case there was no previous history. The girl was apparently well formed, mammae developed, &c. She had died of measles. It is so rare that we can combine in one case somewhat of the previous history and the anatomical character—that perhaps you will pardon me if I quote a case recently recorded by Dr. T. R. Brown of Baltimore.

CASE X.—"I was called," he says, "three months ago to a lady, aged 19, who, four days previous to her death, had quite a profuse epistaxis, lasting about 48 hours, and concluding with a sort of nasal catarrh, which, taken with the facts that she had never menstruated and that the nose-bleedings were frequent in their recurrence, induced an examination *post-mortem* of the organs of generation.

"The vulva was natural in formation and appearance, with the mons veneris and external surface of the labia majora well covered with hair; no clitoris could be perceived. The vagina, which was a simple *cul de sac* about two inches long, was dissected out without encountering the crura clitoridis. It was destitute of rugæ, hymen, and carunculae myrtiliformes, and had no communication with an os uteri. The bladder and rectum were firmly adherent to each other, instead of being separated by a uterus, for which we hunted in vain. The bond of adhesion between bladder and rectum was the broad ligament occupying its usual position, of a crescent shape, and embedded in a thin horn of this crescent, near the summit, about $1\frac{1}{2}$ inch internal to, and on a line with, the iliac fossa, was a nodular body, dense in structure, of the size of an apricot kernel, to which were attached a perfect ovary, Fallopian tube, and round ligament.

"The parts adjacent to the ovaries were greatly congested, evidently connected with a recent ovulation; and an incision into one of the ovaries showed several corpora lutea, with their corresponding cicatrices on the outer surfaces.

"I am of opinion that the nodular bodies referred to were what would correspond to the superior cornua of the uterus, and the non-striated muscular fibre, found in a section, confirms my impression of its being uterine tissue.

"The mammae were unusually well developed, and the symmetry of the figure well illustrated the vigour of her previous health and her

powers of endurance in sickness. Among the many interesting points I will (he adds) refer to two or three.

"1. The anatomical fact, well borne out by this anomaly, that the uterus and vagina are formed by the coalescence of the ducts of Müller, as well as the Fallopian tubes, and the absence of the uterus, as in this case, necessitates the absence of a perfectly-formed vagina.

"2. I ascertained, that notwithstanding there was neither vagina nor clitoris, she had had sexual desires, which circumstances seemed to show had been gratified.

"3. The vicarious menstruation by epistaxis tending to prove that this monthly discharge is necessary to the maintenance of health, irrespective of its point of exit, and is associated, with perhaps very few exceptions, with ovulation."

Let me add myself that this case, by proving that menstrual corpora lutea existed, although there was apparently no receptacle of any kind for them, enables us in some degree to understand the presence of the signs of puberty in cases where the uterus is absent, and that, inferentially, the presence of these signs is an argument for the existence of active ovaries.

CASE XI.—This case differs from all the preceding ones, in not being a case of deficient organ or organs, but of a closed aperture, which I have no doubt was congenital.

Mary —, aged 25, from the country, consulted me some years ago, because she had never menstruated, and also because she had detected a tumour above the pubis. She was fully developed sexually, and the vagina perfectly natural. At its upper extremity was the cervix uteri, much swollen and expanded, and giving the sense to the touch of containing fluid. A shock communicated to the cervix by the finger was plainly felt by the hand placed on the tumour above the pubis. No os uteri could be found. I had little hesitation in deciding that it was a case of imperforate os uteri, and that the tumour was formed by an accumulation of menstrual fluid, as was proved when I punctured the cervix uteri and gave exit to at least a pint of thickened menses. She obtained great relief, but after a time I had to re-open the os uteri, and to keep it from healing by a tent, after which she menstruated regularly and continued perfectly well.

Closure of the vaginal outlet is sufficiently common to make it unnecessary for me to detail the cases which have occurred to me. They presented the usual characters, and were relieved and cured in the usual manner.

I may just remark, however, that I have seen a good many cases of young children under 8 or 10 years where there was apparently no orifice. The closure seemed to be the result of the adhesion of the

opposite edges, and was remedied, without the knife, by breaking through the adhesion with a blunt probe and keeping the sides separate by tent dipped in oil for a few days.

As this is intended for a practical paper, I shall not detain the Society with any theoretical explanation as to the physiological causes of these malformations, but refer them to the different writings upon the subject. Neither have I more to say than I have already said upon the treatment, for in most cases little or none is necessary.

To remedy the undeveloped state of the uterus the late Sir J. Y. Simpson proposed the insertion of a galvanic pessary, and there is evidence of its successful use, but I can add nothing from personal experience.

But there is one question, involving very grave considerations, which is sure to be submitted to us, and our answer to which will require great delicacy, discretion, and firmness—I mean the question whether we think our patient ought to marry—not perhaps whether she is to be allowed to marry, for she may claim to decide that for herself, but her friends, and perhaps she herself, will look to us for guidance in the matter. In such a case as No. 1, no question will arise, for no suspicion existed that all was not right; but in the others it was known. It adds to the painful nature of the question that in some at least sexual desire existed.

Yet it was quite evident that not only could not conception take place, but that the act of intercourse could not be completed, and the great end of marriage must be frustrated.

Moreover, however much we may feel for our patient, and however reluctant to condemn her to a single life, we cannot forget that there is another person concerned, and if out of weak kindness we assent to or advise the marriage, we may entail upon the husband a life-long misery.

I think, therefore, that our decision must be that a girl in such circumstances ought not to be allowed to marry, or if she is bent upon doing so, the other party ought to be fully informed as to the existing defect.

Since this paper was read to the Society the following case has come under my observation, and as it appears to me of great interest, I append it to my paper:

Miss X. Y. Z., aged 19 years, in perfect health, well made, rather short and broad. She has never menstruated, and never felt the usual symptoms of approaching menstruation in the slightest degree. She is in good condition, but the breasts are not more prominent than in a girl of ten years. There is no hair at all on any part of the external genital organs, which are hardly more developed than in a girl of twelve years. The clitoris is very small. The vagina is normal in diameter and length,

and at its upper termination I felt a small cervix, not much larger than the nipple of a gun on which the cap is fixed. I could feel the continuation of the cervix into the body of a very small uterus, not larger than it is before puberty, and this was confirmed by the finger introduced into the rectum. Neither the bi-manual examination, nor that by the finger in the rectum and a catheter in the bladder, added anything to my information. I could find no trace of ovaries. This proves very little, but taken in connexion with the absence of all signs of puberty, one must conclude, I think, that either the ovaries are absent, or, if present, are in an undeveloped, infantile state.

DR. RINGLAND said that Dr. Churchill had truly remarked that few men could be engaged in obstetric practice for some years without encountering some such cases as those he had mentioned. Whilst Dr. Churchill was proceeding with the details of the cases included in his very valuable and instructive paper, a number of analogous cases occurred to his (Dr. Ringland's) mind; but he would not detain the Society with reference to any save one, which was so exceptional in every feature that he thought it merited more than a passing observation. He had had opportunities of mentioning the particulars of this case to several of his professional friends who considered it most remarkable in its several aspects—in fact, almost unique. He had frequently been urged and had intended to mention it to the Society long since. He, consequently, gladly availed himself of the present opportunity of doing so, as it bore so completely on Dr. Churchill's paper.

He was consulted, many years since, by the friends of a young lady in consequence of her intended marriage, and of the persistence of amenorrhœa. She was 20 years of age, and had never menstruated; but for more than four years previously a vicarious discharge had presented itself with tolerable regularity, at each monthly period, for months together, which, however, for months again, would be totally absent. Sometimes it was eliminated from the bladder, at others from the rectum, from the mouth, the nose, or the eyes. She had been under the care of an eminent practitioner in Dublin for a considerable number of years, and he had employed every medicine recommended for the induction of menstruation—in fact, had exhausted the pharmacopœia, but without avail. She now became the patient of Dr. Ringland, who having ascertained the failure of medical remedies, came to the conclusion that something more was needed, and, with this view, suggested the desirability of a minute examination of the genital organs, which was permitted, and which was carefully carried out by the late Dr. Beatty and himself, the young lady having been first placed under the influence of chloroform.

The first thing which struck them was the complete development of the left breast, while the right was that of a girl of not more than twelve years of age. Descending to the pudenda, a similar contrast in the development of the two sides presented itself. There was hair on the left side of the pubes, whilst the opposite side was devoid of any, or only a mere down. There was a fully formed labium on the left side, and scarcely anything analogous to one on the right. There was a total absence of clitoris. On raising the left labium there was no sulcus whatever, only a mere line or *raphè*, extending from before backwards. There was no orifice of a vagina, nor, after a most careful examination of this *raphè*, could they find any depression therein corresponding with that opening; but the meatus urinarius and the anus were in complete proximity. When a sound was passed into the bladder, and a finger into the rectum, the septum between the urethral canal and the intestine was of the thinnest possible character—in fact, scarcely of the thickness of a piece of wet bladder. On passing the finger still further up the rectum, no body presented itself that could represent or be analogous to the uterus. On the contrary, pressure being made above the pubis with the other hand, the finger within the rectum could be distinctly felt therewith without any solid body intervening, clearly indicating that there existed no uterus whatever. Proceeding with their investigation internally, they found an object on the left side, situated in the usual position of, and which they believed to be, an ovary, but on the opposite side there was no trace of anything resembling one. When consciousness returned, after the effects of the chloroform had passed away, the small object on the left side could be distinctly felt through the abdominal parieties, and was somewhat sensitive to pressure.

These appeared to be the remarkable features of the case, and the opinion given by Dr. Beatty and himself was strongly against the advisability of the young lady getting married, and in this it was evident that they had come to a similar conclusion as Drs. Churchill and Kidd had subsequently done in the case detailed in the paper they had just heard. In fact, he believed that no conscientious medical man could arrive at another opinion.

In the case now detailed the father of the young lady most urgently pressed that an operation should be attempted, and the patient herself would have willingly submitted to any that would have offered the least prospect of success; but any such proceeding was strongly advised against and persistently resisted by the medical attendants, because of the immediate proximity and intimate connexion between the urethra and rectum; and that, consequently, there existed no place whatever whereon to perform any operation. This reasoning and conclusion, however, did not satisfy the young lady's friends, who subsequently obtained the opinion of some of the most eminent surgeons of the city on

the subject, but, in every instance, with a like result; and she still, after a lapse of several years, remains unmarried.

Dr. Ringland considered this case an interesting one, and worthy of record, as showing the full development of that side of the female wherein the ovary existed, whilst the opposite side, in which no ovary was to be found, remained altogether undeveloped. He also thought it worthy of note, in a physiological point of view, that, although in this case the clitoris was altogether wanting, yet considerable sexual desire not merely existed but was of frequent recurrence, clearly overthrowing the old theory, and demonstrating that whilst the clitoris is the excitable, the ovary is the exciting organ.

DR. KIDD said he had, as stated by Dr. Churchill, seen one of the cases referred to in his paper. In that case the mode of examination which enabled him to say there was an ovary present was placing the young lady on her back with the legs well drawn up, and with the left hand on the epigastrium making a deep pressure, and with a finger in the rectum he thus felt what he believed was an ovary, and what was proved to be so by examination of a much more serious character performed in London, but which did not give much more information. During this examination, as Dr. Churchill had described, an incision was made in the perinæum where the vulva ought to be, and a finger passed up came into contact with this body which proved to be an ovary. It was of interest to observe that by the bi-manual method of examination described by Marion Sims (with which, however, the profession here was familiar before he had described it), they were able to learn as much as was learned by making the incision. In that case the young lady was very desirous of marrying, and her friends also were very desirous that she should marry. An application was made to him as to whether he could make a vagina for her sufficient to justify her marriage. He said he had no doubt a vagina could be made by a tedious, difficult, and dangerous dissection which would for a short time become practicable, but as soon as surgical treatment ceased it would be closed, and the marriage would only end in dissatisfaction, and it was therefore decided that no attempt should be made. At the same time he saw another lady presenting opposite features. She had all the appearance of a child. She was twenty-seven years of age, but did not look more than eleven, short in stature—she was puny in every respect. He was not allowed to examine the organs of generation. She never menstruated, and from the appearance she presented he gave the opinion there were no ovaries, and advised that there should be no attempt to induce menstruation. There was one point on which he wished to supplement Dr. Churchill's paper, and that was, that in certain cases they might be led into error about the ovaries from their inaction and the inaction of

the uterus. There were certain cases where the uterus remained, as described by Sir James Simpson, in an infantile condition, and in which, by properly directed treatment this organ might become developed and take on its natural functions. He referred to the treatment by the galvanic pessary as described by Simpson. One case which came under his notice was that of a lady seventeen years married who had never menstruated, and had never had children. That lady, suddenly seized with a desire to have children placed herself under medical treatment. He introduced the galvanic pessary, without, however, holding out any hopes to her. It brought on menstruation, and for some months she menstruated regularly while wearing the galvanic pessary. When the wearing of this was given up the menstruation occurred some three or four times but then ceased, and her desire of having a family was not gratified. But there were other cases in younger subjects where the arrest of development was not to such an extent, in which he had no doubt the galvanic pessary persevered in would restore the function of the organ, and establish menstruation. There was another case of a lady who, when she first began to menstruate, not knowing what it was, plunged into a cold bath, and the progress of the menstruation was checked immediately. She afterwards took a cold bath whenever she saw the menstrual fluid making its appearance. She grew up and married, but menstruated not more than once or twice in the year. She was well developed. He treated her with the galvanic pessary. After giving up its use she continued to menstruate every month, but with pain. He then divided the cervix and she was relieved of pain. She became pregnant and had a healthy living child; became pregnant a second time and died of acute atrophy of the liver during that pregnancy. It was a remarkable instance of the success of treatment by means of the galvanic pessary. Therefore, in addition to the question of marriage, the question of treatment was important. In cases where there was a uterus, without distinct evidence of the absence of ovaries, he thought the question of treatment was not to be ignored.

The VICE-PRESIDENT (Dr. Atthill) said the point which Dr. Churchill had especially brought before them, was with reference to marriage in the class of cases under consideration. He quite admitted with Dr. Kidd that it was not necessarily the most important, the question of treatment being sometimes of even greater importance. In the case mentioned by Dr. Ringland the question of marriage was settled. Persons so afflicted should not be permitted to marry. But there were a considerable number of cases in which there was an imperfect development of the organs and the woman might not menstruate; but he should hesitate to prohibit marriage. He was consulted not long since by a married lady in whom the vagina was half an inch, and the uterus one and a half inch long. He believed if that lady had been treated by the use of the galvanic stem

pessary the condition of the uterus would have improved, but she had led for years a very happy married life, and she and her husband however decided that she would not submit to medical treatment of any kind. In this case he believed that the ovaries were perfect, because occasionally a red discharge issued from the vagina, and she experienced on these occasions pains in the breast and in the back. If in a case of this kind in an unmarried female his opinion were to be asked as to the propriety of marriage, he would hesitate to say that she must not marry. He thought the benefit to be derived from the galvanic pessary was not as sufficiently known as it ought to be. It was scouted by persons who had not tested its use indeed. He had heard gentlemen say that the stem pessary could not be introduced, but he had never met a case in which he could not introduce one. His experience went to endorse the practice advocated by Dr. Kidd.

The Society adjourned.

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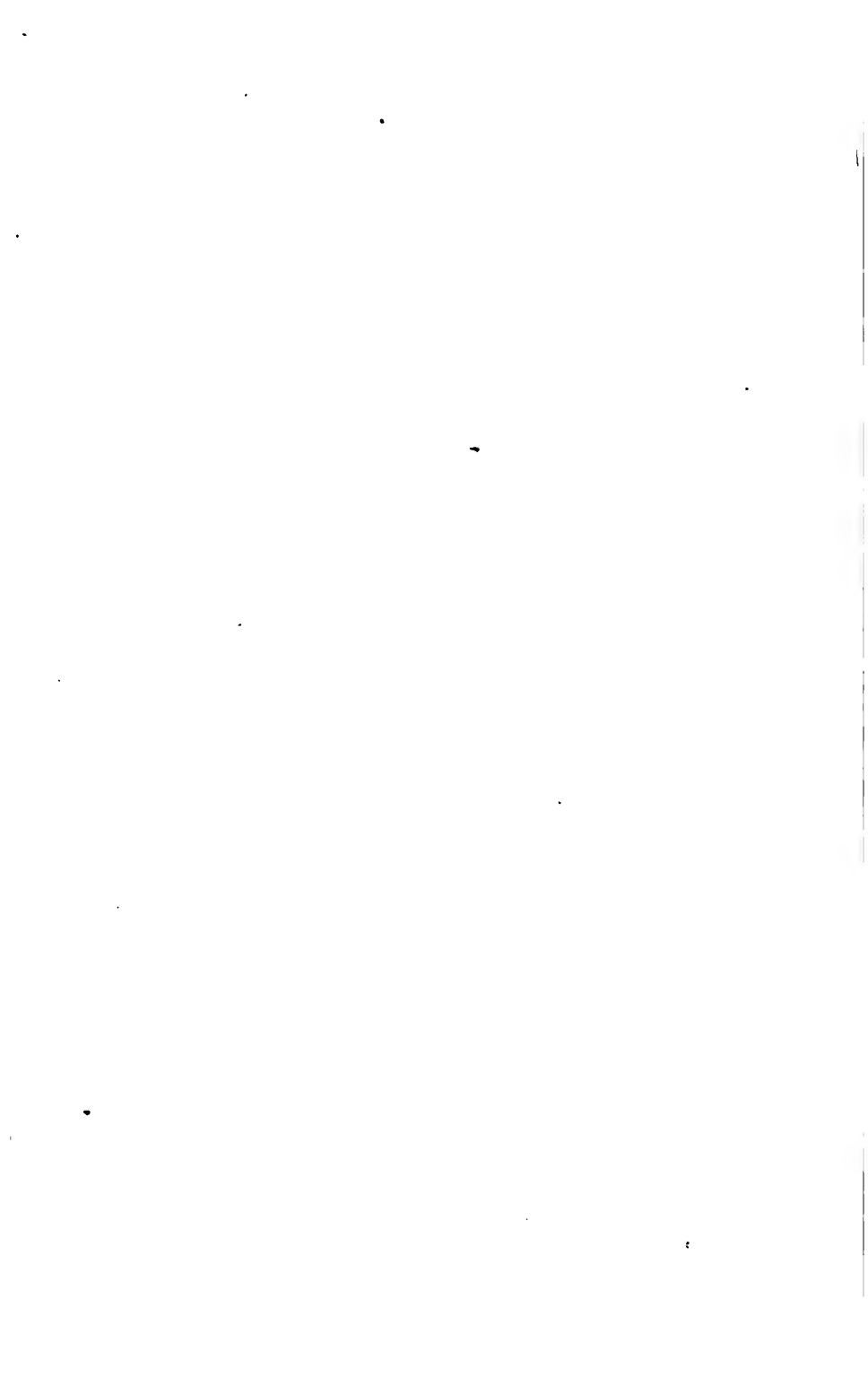
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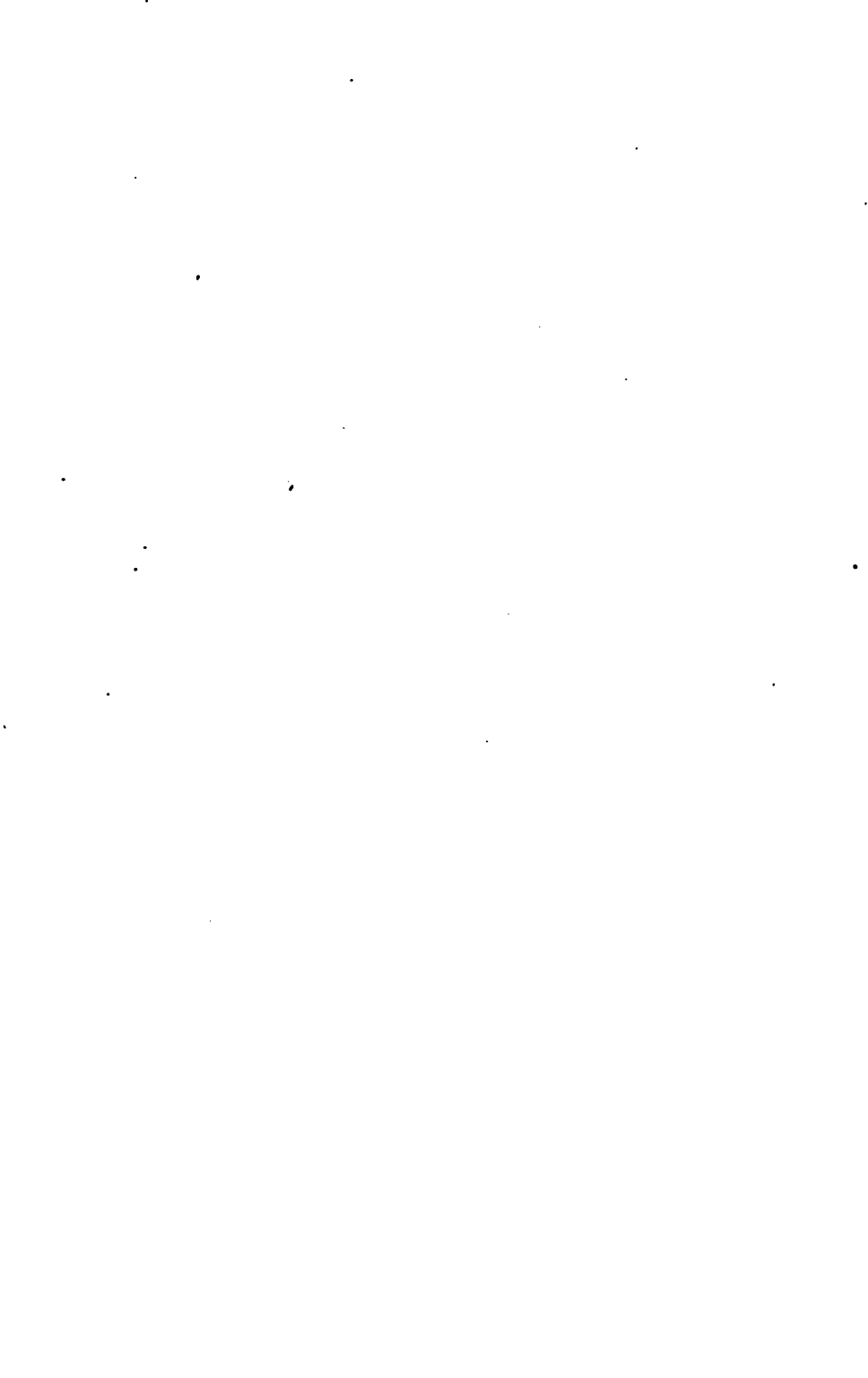
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